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The science of mechanics

Ernst Mach

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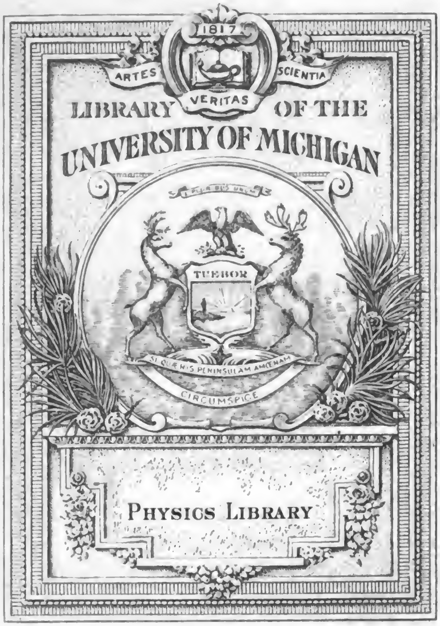

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THE SCIENCE OF MECHANICS

(SUPPLEMENTARY VOLUME)

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THE SCIENCE OF MECHANICS

A CRITICAL AND HISTORICAL ACCOUNT OF ITS DEVELOPMENT

BY

ERNST MACH
EMERITUS PROFESSOR OF THE HISTORY AND THEORY OF INDUCTIVE SCIENCE IN THE UNIVERSITY OF VIENNA

SUPPLEMENT TO THE THIRD ENGLISH EDITION

CONTAINING THE AUTHOR'S ADDITIONS TO THE SEVENTH GERMAN EDITION

TRANSLATED AND ANNOTATED BY

PHILIP E. B. JOURDAIN
M.A.(Cantab.)

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PUBLISHERS' PREFACE TO THE SUPPLEMENTARY VOLUME

THE first edition of the English translation by Mr M'Cormack of Mach's *Mechanics* was published in 1893, and was carefully revised by Professor Mach himself. Since then two other editions of this translation have appeared, in which the alterations contained in the successive German editions have been embodied in the form of appendices. In the seventh German edition, however, which appeared at Leipsic (F. A. Brockhaus) in 1912, there have been more profound modifications in the plan of Professor Mach's work, which are shortly referred to in the preface to that edition. Many things are added and some things are omitted. Among the parts omitted are the prefaces to all of the German editions except the first, and a new preface to the seventh edition has been added. The most extensive additions relate to recent historical researches on the work of Galileo's precursors and the early work of Galileo himself; and the book is dedicated to the late Emil Wohlwill, of whose researches much use has been made.

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In the present English edition, after much thought and consultation with Professor Mach and at the suggestion of Mr Philip E. B. Jourdain, we have adopted a different plan. Mr Jourdain has assumed the responsibility of a revision of the *Mechanics* on the basis of the seventh German edition, and has signified the alterations to text and appendix in the appendix printed here. The only other addition to the seventh German edition is a portrait of Newton after Kneller's well-known picture. This very welcome addition—no portrait of the greatest of mechanical inquirers having adorned previous editions of the *Mechanics*—is also given as the frontispiece of the present volume. The reader who possesses the third English edition of the *Mechanics*[1] as well as this volume has a complete picture of the various stages through which Mach's *Mechanics* has passed.

And this retention of the successive alterations and additions seems almost to be necessary. Indeed, Mach's work is to be regarded not only as a contribution to the enlightenment of so many points in the history and the principles of mechanics, but also as a foundation-stone of science, which is of the greatest historical interest in itself. The slow but sure progress of digestion of Mach's ideas—which

[1] *The Science of Mechanics: A Critical and Historical Account of its Development*, translated by T. J. M'Cormack, third edition; Chicago and London: The Open Court Publishing Company, 1907.

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must have seemed so revolutionary to most of our modern schoolmen—and the ever-growing influence of these ideas on teaching are both reflected in these prefaces. And where Mach's historical knowledge has grown—and grown, it is to be observed, in consequence of the researches of others who were often inspired by Mach's own work,—it is surely of absorbing interest still to be able to read the original form of Mach's work and compare it with the later emendations.

At the end of these appendices, Mr Jourdain has added some notes of his own which Professor Mach has commended in his preface to the seventh German edition. Any other notes which have been added to the text of Mach's appendix for the purpose of completing or correcting references, or of referring to more generally accessible editions or translations of the works cited by the author, are enclosed in square brackets.

In spite of many obstacles and inconveniences, occasioned mainly by the inability to use his right hand, Professor Mach has most kindly revised the entire work of Mr Jourdain, including all additions and alterations.

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AUTHOR'S PREFACE TO THE SEVENTH GERMAN EDITION

WHEN, forty years ago, I first expressed the ideas explained in this book, they found small sympathy, and indeed were often contradicted. Only a few friends, especially Josef Popper the engineer, were actively interested in these thoughts and encouraged the author. When, two years later, Kirchhoff published his well-known and often-quoted dictum, which even to-day is hardly correctly interpreted by the majority of physicists, people liked to think that the author of the present work had misunderstood Kirchhoff. I must decline with thanks this, as it were, prophetical misunderstanding as not corresponding either to my faculty of presentiment or to my powers of understanding.

However, the book has reached a seventh German edition, and by means of excellent English, French, Italian, and Russian translations has spread over almost all the world. Gradually some of those who work at this subject, like J. Cox, Hertz, Love, MacGregor, Maggi, H. von Seeliger, and others, gave voice to their agreement. For them, of course, only details in a book meant for a general intro-

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duction could be of interest. In this subject, I could hardly avoid touching upon philosophical, historical, and epistemological questions; and by this the attention of various critics was aroused. I took special joy in the recognition which I found with the philosophers R. Avenarius, J. Petzoldt, H. Cornelius, and, later, W. Schuppe. The apparently small concessions which philosophers of another tendency, like G. Heymans, P. Natorp, and Aloys Müller, have granted to my characterisation of absolute space and absolute time as misconceptions suffice for me; indeed, I do not wish for anything more. I thank Messrs L. Lange and J. Petzoldt not only for their agreement in certain details, but also for their active and fruitful collaboration. In a historical respect, the criticisms of Emil Wohlwill, whose death, I regret to say, has just been announced to me, were valuable and enlightening to me, especially on the period of Galileo's youthful work; further, critical remarks of P. Duhem and G. Vailati have also been valuable. I am very grateful to Mr Philip E. B. Jourdain of Cambridge for his critical notes that unfortunately, for the most part, came too late for inclusion in *this* edition, which was already nearly finished. P. Duhem, O. Hölder, G. Vailati, and P. Volkmann have taken part in the epistemological discussions with vigour, and their remarks have been helpful to me.

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At the end of the last century my disquisitions on mechanics fared well as a rule; it may have been felt that the empirico-critical side of this science was the most neglected. But now the Kantian traditions have gained power once more, and again we have the demand for an *a priori* foundation of mechanics. Now, I am indeed of the opinion that all that can be known *a priori* of an empirical domain must become evident to mere logical circumspection only after frequent surveys of this domain, but I do not believe that investigations like those of G. Hamel[1] do any harm to the subject. Both sides of mechanics, the empirical and the logical side, require investigation. I think that this is expressed clearly enough in my book, although my work is for good reasons turned especially to the empirical side.

I myself—seventy-four years old, and struck down by a grave malady—shall not cause any more revolutions. But I hope for important progress from a young mathematician, Dr Hugo Dingler, who, judging from his publications,[2] has proved that he has attained to a free and unprejudiced survey of *both* sides of science.

This edition will be found somewhat more homo-

[1] "Über Raum, Zeit und Kraft als apriorische Formen der Mechanik," *Jahresber. der deutschen Mathematiker-Vereinigung*, vol. xviii, 1909; "Über die Grundlagen der Mechanik," *Math. Ann.*, vol. lxvi, 1908.

[2] *Grenzen und Ziele der Wissenschaft*, 1910; *Die Grundlagen der angewandten Geometrie*, 1911.

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geneous than the former ones. Many an ancient dispute which to-day interests nobody any more is left out and many new things are added. The character of the book has remained the same. With respect to the monstrous conceptions of absolute space and absolute time I can retract nothing. Here I have only shown more clearly than hitherto that Newton indeed spoke much about these things, but throughout made no sérious application of them. His fifth corollary[1] contains the only practically usable (probably approximate) *inertial system.*

ERNST MACH.

VIENNA, *February 5th,* 1912.

[1] *Principia,* 1687, p. 19.

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THE SCIENCE OF MECHANICS

APPENDIX OF ADDITIONS AND ALTERATIONS TO THE SEVENTH GERMAN EDITION

I

[To p. 515, line 9 of third edition of *Mechanics*, add :]

I must here draw my readers' attention to a beautiful paper by G. Vailati,[1] in which the side of Hölder against my criticism of Archimedes' deduction of the law of the lever is taken, but partly too Hölder is criticised. I believe that everyone may read Vailati's exposition with profit and, by comparison with what I have said on pp. 17–20 of the third edition of my *Mechanics*, will be in a position himself to form a judgment upon the points at issue. Vailati shows that Archimedes

[1] "La dimostrazione del principio delle leva data da Archimede," *Bolletino di bibliografia e storia delle scienze matematiche*, May and June 1904.

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derives the law of the lever on the basis of general experiences about the centre of gravity. I have never disputed the view that such a process is possible and permissible and even very fruitful at a certain stage of investigation, and further, is perhaps the only correct one at that stage. On the contrary, by the manner in which I have exposed the derivations of Stevinus and Galileo, which were made after the example of Archimedes, I have expressly recognised this. But the aim of my whole book is to convince the reader that we cannot make up properties of nature with the help of self-evident suppositions, but that these suppositions must be taken from experience. I would have been false to this aim if I had not striven to disturb the impression that the general law of the lever could be deduced from the equilibrium of equal weights on equal arms. I had, then, to show where the experience that already contains the general law of the lever is introduced. Now this experience lies in the supposition emphasised on p. 14, and in the same way it lies in every one of the general and undoubtedly correct theorems on the centre of gravity brought forward by Vailati. Now, because the fact that the value of a load is proportional to the arms of the lever is not directly and in the simplest way apparent in such an experience, but is found in an artificial and roundabout way, and is then offered to the surprised reader, the modern

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reader has to object to the deduction of Archimedes. This deduction from simple and almost self-evident theorems may charm a mathematician who either has an affection for Euclid's method, or who puts himself into the appropriate mood. But in other moods and with other aims we have all the reason in the world to distinguish in value between getting from one proposition to another and conviction, and between surprise and insight. If the reader has derived some usefulness out of this discussion, I am not very particular about maintaining every word I have used.

II

[To p. 49, line 2, add :]

In my exposition in the preceding editions, E. Wohlwill finds that the achievements of Stevinus are over-estimated as compared with those of del Monte and Galileo. In fact, del Monte, in his *Mechanicorum liber* (Pisauri, 1577), considered the lengths of the paths which are described simultaneously by the weights in the cases of the lever, pulleys, and wheel and axle. His consideration is more geometrical than mechanical. Also, with del Monte is lacking the principle by which the surprising character is taken away from the effects of machines (*cf.* Wohlwill, *Galilei*, i, pp. 142 *et seqq.*). Thus del Monte was out-distanced by other mediæval writers who concerned themselves

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with the heritage of the principle of virtual velocities which had been handed down by the ancients, and who are to be mentioned on another occasion. Now, at the end of the sixteenth century, Stevinus did *not* advance beyond his immediate predecessor del Monte.

III

[To p. 52, line 2, add :]

E. Wohlwill emphasises that Galileo laid stress on the loss of velocity which corresponds to the economy of force in machines (*cf. Galilei*, i, pp. 141, 142). If we use the modern conception—to the development of which Galileo contributed so much—of "work," we can say without equivocalness : in machines *work* is not economised.

IV

[To p. 85, last line, add :]

The knowledge of the development of a science rests on the study of writings in their historical sequence and in their historical connection. For ancient times many sources are, of course, lacking, and for other times the author is unknown or doubtful. In later centuries, especially before the discovery of printing, the bad habit is general of the author seldom referring to his predecessors where he uses their works, and usually only doing so where he thinks he has to contradict those

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predecessors. By these circumstances, the above study is made very difficult and makes the highest demands on criticism.

P. Duhem develops in his book, *Les origines de la statique* (Paris, 1905, vol. i), the view that E. Wohlwill had already taken, that modern scientific civilisation is much more intimately connected with ancient scientific civilisation than people usually suppose. The scientific thoughts of the Renascence developed very slowly and gradually from those of ancient Greece, particularly from those of the peripatetic and Alexandrian school. I will here emphasise that Duhem's book contains a mine of stimulating, instructive, and enlightening details condensed in a small space. To the knowledge of these details we could only otherwise attain by a wearisome study of old books and manuscripts. By that alone the reading of Duhem's work excites much admiration and is very fruitful.

In especial, Duhem ascribes to Jordanus Nemorarius, a writer of the thirteenth century who was an interpreter and developer of ancient thoughts, and to a later elaborator of the *Liber Jordani de ratione ponderis*, whom he calls the "forerunner of Leonardo da Vinci," a great influence on Leonardo, Cardano, and Benedetti. The most important corrections to *Jordani opusculum de ponderositate*, which Tartaglia published as his own and used in *Questi et inventioni*

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diverse without naming Jordanus or his later elaborator, are contained in a manuscript under the title *Liber Jordani de ratione ponderis*, which Duhem found in the national library at Paris (*fond latin*, No. 7378 A). This leads to the supposition of the anonymous "forerunner." Also, Leonardo's manuscripts, which were not carefully preserved and were unprotected from unauthorised use, have had, according to Duhem, in spite of their delayed publication, an effect on Cardano and Benedetti. The authors named above influenced, above all, Galileo in Italy, Stevinus in Holland, and their works reached France by both channels. There they found, in the first place, fruitful soil in Roberval and Descartes. Consequently, the continuity between ancient and modern statics was never broken.

Let us now consider some details. The author of the *Mechanical Problems* mentioned on p. 511 remarks about the lever that the weights which are in equilibrium are inversely proportional to the arms of the lever or to the arcs described by the endpoints of the arms when a motion is imparted to them.[1] With great freedom of interpretation we can regard this remark as the incomplete expression

[1] According to the view of E. Wohlwill, it may be considered to be decided that the *Mechanical Problems* cannot be due to Aristotle. *Cf.* Zeller, *Philosophie der Griechen*, 3rd ed., pt. ii, § ii, note on p. 90. But then a thorough investigation as to whether the lately found Arabic translation (published in 1893) of Hero's *Mechanics*, if not the older text, is necessary. *Cf.* Heron's *Werke*, edited by L. Nix and W. Schmidt (Leipsic, 1900), vol. ii.

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of the principle of virtual displacements. But, with Jordanus Nemorarius (Duhem, *op. cit.*, pp. 121, 122), the equilibrium of the lever is characterised by the inverse proportionality of the height to which the weights are raised (or the depths to which they fall) to the weights which are in equilibrium. The essential point is brought into prominence by this. Jordanus also knows that a weight does not always act in the same way, and introduces—though only qualitatively—the conception of weight according to position: "secundum situm gravius, quando in eodem situ minus obliquus est descensus" (*op. cit.*, p. 118). The "forerunner" of Leonardo improves and completes the exposition of Jordanus. He recognises the equilibrium of an angular lever whose axis lies above the weights, by the consideration of the possible depths of falling and heights of rising, as *stable* (*op. cit.*, p. 142). He knows also that such a lever directs itself in such a manner that the weights are proportional to their distances from the vertical through the axis (*op. cit.*, pp. 142, 143), and thus arrives in essentials at the use of the conception of *moment*. The "gravitas secundum situm" thus here attains a *quantitative* form and is used in a brilliant way for the solution of the problem of the inclined plane (*op. cit.*, p. 145). If two weights on inclined planes of equal heights but different lengths are so connected by a rope and pulley that the one must rise when the other sinks, the weights are,

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in the case of equilibrium, inversely as the *vertical* displacements, that is to say, vary directly as the lengths of the inclined planes. Consequently in this the "forerunner" anticipated the essential elements of modern statics.

The study of the manuscripts of Leonardo, which have only been published in part, is extremely profitable. The comparison of his various occasional notes shows clearly his knowledge of the principle of virtual displacements, or rather of the concept of work, though he does not use any special nomenclature. "When a force carries (raises?) a body (a weight?) in a certain time through a definite path, the same force can carry (raise?) half of the body (the weight?) in the same time through a path double in length." This theorem is applied to machines, lever, pulleys, and so on, and by this the rather doubtful meaning of the above words is more closely determined. If we have a definite quantity of water which can sink to a definite depth, we can, according to Leonardo, drive one or even two equal mills with it, but in the second case we can only accomplish as much as in the first case. The perception of the "potential lever," to which Leonardo attained by a stroke of genius, put him in the position to gain all the insight which was reached later by the conception of "moment." His figures make us suspect that the consideration of the pulley and the wheel and axle showed him the way

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to his conception (*cf. Mechanics*, p. 20). Leonardo's constructions concerning the pulls on combinations of cords visibly rest, too, on the thought of the potential lever. Leonardo was less happy in the treatment of the problem of the inclined plane. By the side of sketches in which sometimes a correct view is expressed, we find many incorrect constructions. However, we must consider Leonardo's scribblings as leaves of a diary, which fix the most various sudden ideas and points of view and beginnings of investigations, and do not attempt to carry out these investigations according to a unitary principle. To explain the fact that Leonardo was not master of all the problems which had been completely solved in the thirteenth century, we must remember that it by no means suffices, as we must recognise with Duhem, that an insight should be once attained and made known, but years and centuries are often necessary for this insight to be *generally* recognised and understood (Duhem, *op. cit.*, p. 182).

The idea of the impossibility of perpetual motion is developed with Leonardo to great clearness. The consideration about the mill shows this: "No impetus without life can press or draw a body without accompanying the body moved; these impetuses can be nothing else than forces or gravity. When gravity presses or draws, it effects motion only because it strives for rest; no body can, by its motion of falling, rise to the height from which it fell; its motion

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reaches an end" (*op. cit.*, p. 53). "Force is a spiritual and invisible power which is impregnated in bodies by motion (here we certainly have to think of what at the present time is called *vis viva*); the greater it is the more quickly does it expend itself" (*op. cit.*, p. 54). Cardano has a similar view in which we may judge an influence of Leonardo to be probable if we have grounds for doubting Cardano's independence (*op. cit.*, pp. 40, 57, 58). Also, Aristotle's idea that only the circular motion of the heavens is eternal appears again with Cardano. Duhem considers that Cardano is not a common plagiarist. He used indeed without acknowledgment the works of his predecessors, especially those of Leonardo, but brought these works into a better connection and, by that, improved the position of the sixteenth century (*op. cit.*, pp. 42, 43). Cardano does not overcome the problem of the inclined plane; his opinion is that the weight of the body on the inclined plane is to the whole weight as the angle of elevation of the plane is to a right angle. Benedetti put himself in opposition to all his predecessors, and this opposition had a good effect, especially in criticism of the dynamical doctrines of Aristotle. But Benedetti was often opposed to what was right. In his writings occur again thoughts of Leonardo's, and errors of Leonardo's as well.

If we regard the discoveries we have just spoken of as sufficiently known and accessible to the suc-

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cessors of the above men, there remains for these successors—especially for Stevinus and Galileo—not very much more to do in statics. Stevinus's solution of the problem of the inclined plane (*cf. Mechanics*, pp. 24–31) is indeed quite original, but the "fore-runner" of Leonardo already knew the *result* of the considerations of Stevinus and Galileo, and Galileo's considerations join on to those of Cardano. From the consideration of the inclined plane Stevinus attained to the composition and resolution of *rectangular* components according to the principle of the parallelogram, and considered this principle to be generally valid without being able to prove it. Roberval filled up this gap. He imagined a weight R supported by pulleys and held in equilibrium by a cord of any direction loaded with counter-weights P and Q. If, first, we consider one cord as a rod which can rotate about the pulley and apply Leonardo's principle of the potential lever, and then proceed in a similar way with respect to the other cord, we find the relations of R to P and Q and all the theorems which hold for the triangle of forces or the parallelogram of forces (*op. cit.*, esp. p. 319). Descartes finds in the principle of virtual displacements the foundation for the understanding of *all* machines. He sees in work, the product of weight and distance of falling (in his nomenclature, "force"), the determining circumstance or cause of the behaviour of machines, the *Why* and not

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merely the *How* of the event. It is not a question of the velocity, but of the height of raising and the depth of falling. "For it is the same thing to raise a hundred pounds two feet or two hundred pounds one foot" (*op. cit.*, p. 328; *cf.* p. 54 of *Mechanics* on Pascal's statement). Descartes denies the unmistakable influence on his thoughts of all his predecessors from Jordanus to Roberval; and yet his developments show everywhere important progress, and throughout he emphasises essential points (*op. cit.*, pp. 327–352).

With respect to details we must refer to Duhem's brilliant book. Here I will only give expression to my somewhat different opinion on the relation of ancient to modern natural science. Natural science grows in two ways. In the first place, it grows by our retaining in memory the observed facts or processes, reproducing them in our presentation, and trying to reconstruct them in our thoughts. But, as the observations are continued, these attempts at construction, which are successively or simultaneously taken in hand, always show certain defects by which the agreement of these constructions both with the facts and with one another is disturbed. Thus there results a need for material correction and logical harmonisation of the constructions. This is the *second* process which builds up natural science. If everyone had only himself to rely on, he would have to begin anew with his observations

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and thoughts alone, and consequently could not get far. This holds both for single human beings and for single nations. Thus we cannot treasure highly enough the heritage which our immediate predecessors in civilisation—the Greek students of nature, astronomers and mathematicians—have bequeathed to us. We enter on investigation under favourable conditions, since we are in possession of an image of the world,—although this image be insufficient—and are, above all, equipped with the logical and critical education of the Greek mathematicians. This possession makes the continuance of the work easier for us. But we must consider not only our scientific heritage but also *material* civilisation—in our special case the machines and tools which have been handed down to us as well as the tradition of their use. We can easily set up observations on this material heritage, or repeat and extend those which led the investigators of ancient times to their science, and thus for the first time learn really to understand this science. It appears to me that this material heritage—continually waking up anew, as it does, our independent activity—is too little esteemed in comparison with the literary heritage. For can we suppose that the paltry remarks of the author of the *Mechanical Problems* about the lever, and even the far more exact remarks of the Alexandrian mathematicians, would not have continually obtruded themselves upon the

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observing men who were busied with machines, even if these remarks were not preserved in writing? Does not this hold good, say, about the knowledge of the impossibility of perpetual motion, which must present itself to everybody who does not seek wonder in mechanics, as a dreamer after the fashion of the alchemists, but is busied, as a calm investigator, in practice with machines? Even when such finds are transferred to those who come after, they must be gained independently by these followers. The sole advantage a follower has consists in the start that he has gained by a quicker passage over the same course, by which he outstrips his predecessors. An incomplete knowledge put into words forms a relatively firm prop for fleeting thoughts, from which the thoughts, seeking among facts, set out, and to which, modifying it by criticism and comparison, they continually return. Now, whether these props are made stronger by newer experience or are gradually shifted, or are even at last recognised as invalid, they have helped us on. But if the predecessor becomes a great authority, and if even his errors are prized as marks of deep insight, we get a state of things which can only act in a hurtful way on the followers of this man. Thus, by many passages in the writings of E. Wohlwill and P. Duhem, it seems that even Galileo was sometimes hindered, even in his later years, by the traditional peripatetic burden from perceiving undisturbed his

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own far stronger light. In our estimation of the importance of an investigator, then, it is only a question of what *new* use he has made of old views and under what *opposition* of his contemporaries and followers *his own* views have come to be held. From this point of view, Duhem seems to me to go rather too far in his feeling of reverence towards the memory of Aristotle. With Aristotle (*De coelo*, book iii, 2) there are, for example, among unclear and unpromising utterances, the passages: "Whatever the moving force may be, the less and the lighter receive more motion from the same force. . . . The velocity of the less heavy body will be to that of the heavier body as the heavier to the lighter body." If we disregard the fact that Aristotle cannot be credited with a clear distinction of path, velocity, and acceleration, we can recognise in this the expression of a primitive but correct experience which led at length to the conception of mass. But, after what we have said in the whole of the second chapter, it seems hardly thinkable to refer this passage to the raising of weights by machines, to combine it with what Aristotle has said about the lever, and then to see in it the germ of the conception of work (Duhem, *op. cit.*, pp. 6, 7; *cf.* Vailati, *Bolletino di bibliografia e storia di scienze matematiche*, Feb. and March, 1906, p. 3). Further, Duhem blames Stevinus for his peripatetic tendencies. But Stevinus seems to me to be in the right when

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he puts himself in opposition to the "wonderful" circles of Aristotle, which are not described in the case of equilibrium. This is just as justifiable as the protest of Gilbert and Galileo against the hypothesis of the effectiveness of a mere position or a point (see *Mechanics*, p. 533). Only from a broader point of view, when work is recognised as that which determines motion, does the dynamical derivation of equilibrium attain the merit of greater rationality and generality. Before that, hardly anything could be urged against Stevinus's inspired deductions on the grounds of instinctive experience and after the manner of Archimedes.

V

[To p. 112, last line of paragraph 1, add :]

To form some idea of the slowness with which the new notions about air became more familiar to men, it is enough to read the article on air which Voltaire,[1] one of the most enlightened men of his

[1] [Voltaire's article "Air" in the first volume of his *Questions sur l'Encyclopédie par des Amateurs* was republished in the *Collection complette des Œuvres de Mr de . . .* (vol. xxi, Geneva, 1774, pp. 73–81; the part noticed in the text above, which contains Voltaire's own opinions, is on pp. 77–79). The *Questions* were first published in 1770–72 in seven volumes, and the article "Air" is in the first part (1770). The *Dictionnaire Philosophique* was first published in 1764, and was greatly augmented in various subsequent editions from 1767 to 1776. The editor, de Kehl, in 1785–89, included various works under the single title of *Dictionnaire Philosophique*, viz., the *Dictionnaire Philosophique*, the *Questions*, a manuscript dictionary entitled *L'Opinion par l'Alphabet*, Voltaire's articles in the great *Encyclopédie*, and several articles destined for the *Dictionnaire de l'Académie Française*. The article "Air" is contained in vol. xxvi of M. Beuchot's *Œuvres de Voltaire* (72 volumes, Paris, 1829), pp. 136–147.]

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time, wrote in his *Dictionnaire Philosophique* from the *Encyclopédie*, in 1764—a century after Guericke, Boyle, and Pascal, and not long before the discoveries of Cavendish, Priestley, Volta, and Lavoisier, —that air is not visible and, quite generally, is not perceptible; all the functions that we ascribe to the air can be discharged by the perceptible exhalations whose existence we have no grounds for doubting. How can the air enable us to hear the different notes of a melody simultaneously? Air and æther are, with respect to the certainty of their existence, put on the same level.

VI

On p. 128 of the *Mechanics*, the words "Dynamics was founded by Galileo," and "Only by traces, which were for the most part mistaken, do we find that their thought extended to dynamics," and on pp. 128–129, the words "and that . . . inquiry" are omitted.

VII

[To p. 129, line 2, add:]

Besides, the views of Aristotle found opponents even in antiquity. Especially the Aristotelian opinion that the continued motion of a body which is projected is brought about by means of the *air* which has been set in motion at the same time plainly showed an obvious point of attack to criticism. According to Wohlwill's researches, Philoponos, a

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writer of the sixth century of the Christian era, expressly contested this view—a view contrary to every sound instinct. Why must the moving hand touch the stone at all if the air manages everything? This natural question asked by Philoponos did not fail to exercise an influence on Leonardo, Cardano, Benedetti, Giordano Bruno, and Galileo. Philoponos also contradicts the assertion that bodies of greater weight fall more quickly, and refers to observation. Finally, Philoponos shows a modern trait in that he denies any force to the *position in itself*, but attributes to bodies the effort to preserve their order (*cf.* Wohlwill, "Ein Vorganger Galilei's im 6. Jahrhundert," *Physik. Zeitschrift von Riecke und Simon*, 7. Jahrg, No. 1, pp. 23–32).

VIII

[After "gravity" on line 1 of p. 521, insert passage, which is *partly* given on p. 521 :]

Just so is the increasing of the projectile-force of a stone by the thrower reduced to an aggregation of impulses. Such an impulse has, according to Benedetti, the tendency to force the body forward in a straight line. A body projected horizontally approaches the earth more slowly; consequently, the gravity of the earth appears to be partly taken away. A spinning top does not fall, but stands on the end of its axis, because its parts have the tendency to fly

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away tangentially and perpendicularly to the axis, and by no means to approach the earth. Benedetti ascribes the continued motion of a projected body not to the influence of the air but to a "virtus impressa," but does not attain to full clearness with respect to the problems (G. Benedetti, *Sulle proporzioni dei motu locali*, Venice, 1553; *Divers. speculat. math. et physic. liber*, Turin, 1585).

Galileo, in the works of his youth, which was spent in Pisa, appears, as has become known by the recent critical edition of his works, as an opponent of Aristotle, as doing honour to the "divine" Archimedes, and as the immediate follower of Benedetti, whom he follows both in the manner in which he puts questions to himself and often in the way of writing, without, however, citing him. Like Benedetti, he supposes a gradually decreasing "vis impressa" in cases of projection. If the projection is upwards, the impressed force is a transferred "lightness"; as this lightness decreases, the gravity receives an increasing preponderance directed below, and the motion of falling is accelerated. In this idea Galileo encounters the ancient astronomer Hipparchus of the second century B.C., but does not do justice to Benedetti's view of the acceleration of falling. For, according to Hipparchus and Galileo, the motion of falling would have to be *uniform* when the impressed force is wholly overcome.

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IX

[Note to p. 129, line 2 up. From this to p. 130, line 10, is omitted, and the passage added :]

In the former editions of this book, the exposition of Galileo's researches was based on his final work, *Discorsi e dimostrazioni matematiche* of 1638.[1] However, his original notes, which have become known later, lead to different views on his path of development. With respect to these I adopt, in essentials, the conclusions of E. Wohlwill (*Galilei und sein Kampf für die Kopernikanische Lehre*, Hamburg and Leipsic, 1909). In the riper and more fruitful time of his residence in Padua, Galileo dropped the question as to the "why" and inquired the "how" of the many motions which can be observed. The consideration of the line of projection and its conception as a combination of a uniform horizontal motion and an accelerated motion of falling enabled him to recognise this line as a parabola, and consequently the space fallen through as proportional to the square of the time of falling. The statical investigations on the inclined plane led to the consideration of falling down such a plane, and also to the observation of the vibrating pendulum. From comprehensive observations and experiments on the

[1] [There is a convenient German annotated translation of the *Discorsi e dimostrazioni matematiche* by A. J. von Oettingen in *Ostwald's Klassiker der exakten Wissenschaften*, Nos. 11, 24, 25; and an English translation by Henry Crew and Alfonso de Salvio under the title *Dialogues concerning Two New Sciences*, New York, 1914.]

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pendulum it appeared that a body which falls down a series of inclined planes can, by means of the velocity thus obtained, rise on any series of other planes to the original height and no higher. In other words, the velocity obtained by the falling only depends on the distance fallen through. Finally, Galileo reached a definition of uniformly accelerated motion which has the properties of the motion of falling, and from which, inversely, all those provisional lemmas which led him to his view can be deductively derived.

With respect to the definition of uniformly accelerated motion, Galileo hesitated for a long time. He first called that motion uniformly accelerated in which the increments of velocity are proportional to the lengths of path described; he held, according to a fragment dating from 1604 (*Edizione Nazionale*, vol. viii, pp. 373–374), and a letter to Sarpi written at the same time, that this conception corresponded to all facts, in which, however, he was mistaken. According to Wohlwill, it was probably about 1609 that he overcame the error and defined uniformly accelerated motion by the proportionality of the velocity to the *time* of motion. He then turned away from his first view on grounds just as insufficient as those on which he had accepted it earlier. The natural explanation of all this will, as in the older editions of this book, be spoken of later. We will now consider what heritage Galileo left to modern thinkers.

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Here it will appear clearly that he allowed himself to be led by suppositions which to-day can be conceived as more or less immediate corollaries from his law of falling; and this perhaps speaks most eloquently for his talent as an investigator and for his discoverer's instinct. Now, whether Galileo attained to knowledge of the uniformly accelerated motion of falling by consideration of the parabola of projection or in another way, we cannot doubt that he tested the law of falling experimentally *as well.* Salviati, who represents Galileo's doctrines in the *Discorsi*, assures us of his repeatedly taking part in experiments, and describes the experiments very accurately (*Le opere di Galilei, Edizione Nazionale*, vol. viii, pp. 212–213).

X

[To p. 527, line 25, add :]

If, now, we ask what views into the nature of things Galileo has bequeathed to us, or at least facilitated in a lasting manner by classically simple examples, we find :

(1) The emphasis upon the conception of work in a statical connection. There is no saving work with machines ;

(2) The advancement of the conception of work in a dynamical connection. The velocity attained by falling, when resistance is neglected, only depends on the distance fallen through ;

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(3) The law of inertia;

(4) The principle of the superposition of motions.

Galileo's creative activity extends far beyond the limits of mechanics; we will only call to mind his founding of thermometry, his sketch of a method for the determination of the velocity of light,[1] his direct proof of the numerical ratio of the vibrations of the musical interval and his explanation of synchronous vibrations. He heard of the telescope, and that was enough for him to rediscover and to improvise one with two lenses and an organ-pipe. In quick succession he discovered, by the help of his instrument, the mountains of the moon—whose height he measured,—Jupiter with his satellites—a small model of the solar system,—the peculiar form of Saturn, the phases of Venus, and the spots and rotation of the sun. These were new and very strong arguments for Copernicus. Also his thoughts on geometrically similar animals and machines and on the form and firmness of bones must be considered to be stimuli to the development of new mathematical methods. Besides Wohlwill, E. Goldbeck ("Galilei's Atomistik und ihre Quellen," *Biblioth. Math.*, 3rd series, vol. iii, 1902, part i) has recently shown that this revolutionising thinker was not wholly independent of ancient and mediæval influences. In particular, the first day of the *Discorsi*

[1] [See Mach's *Popular Scientific Lectures*, 3rd ed., Chicago and London, 1898, pp. 50–54.]

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contains a lengthy exposition of Galileo's atomistic reflections which clearly stand in opposition to Aristotle, and as clearly approximate to Hero's position. These reflections led him to extraordinary discussions on the continuum and to speculations, in which mysticism and mathematics were combined, on the finite and the infinite, which remind us, on the one hand, of Nicolas of Cusa, and, on the other hand, of many modern mathematical researches which are hardly free from mysticism.[1] That Galileo could not attain complete clearness in all his thoughts need surprise us no more than his occupation with paradoxes, whose disturbing and clarifying force every thinker must have experienced.

With respect to the knowledge of accelerated motion Galileo has done the greatest service. For the sake of completeness we will refer to P. Duhem's researches ("De l'accélération produite par une force constante; notes pour servir à l'histoire de la dynamique," *Congrès international de philosophie*, Geneva, 1905, p. 859). Without entering into the many historically interesting details communicated by Duhem, we will here only add the following. According to the literal Aristotelian doctrine, a con-

[1] [See the German translation of the first two days of the *Discorsi* in *Ostwald's Klassiker*, No. 11 (the other days are translated in Nos. 24 and 25), especially pp. 30–32. Besides the article of Goldbeck mentioned in the text above, there is an article by E. Kasner on "Galileo and the Modern Concept of Infinity," which is noticed in the *Jahrbuch über die Fortschritte der Mathematik*, vol. xxxvi, 1905, p. 49. See also Crew and de Salvio's translation of the *Discorsi*, pp. 26–40.]

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stant force conditions a constant velocity. But since the increasing velocity of falling can hardly escape even rough observations, the difficulty arises of bringing this acceleration into harmony with the doctrine that held the field. On approaching the ground, the body, in the opinion of Aristotle, becomes heavier. The traveller hastens when approaching his destination, as Tartaglia expresses it. The air which at one time was viewed as a hindrance and at another time as a motive power must, in order to make the contradictions more supportable, play at one time the one part and at another time the other. The hindering space of air between the body and the ground is, according to the commentator Simplicius, greater at the beginning of the motion of falling than at the end of this motion. The "forerunner" of Leonardo found that air which has once been set in motion is less of a hindrance for the body moved. The naïf observer of a stone projected obliquely or horizontally and describing an initial line which is almost straight must receive the natural impression that gravity is removed by the impulse to motion (see above, Appendix VIII). Hence the distinction between natural and forced motion. The considerations of Leonardo, Tartaglia, Cardano, Galileo, and Torricelli on projectiles showed how the idea of an alteration of two motions which were considered to be fundamentally different gradually yields to that of a mixture and simultaneity of them. Leonardo

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was acquainted with the accelerated motion of falling, and conjectured the increase of velocity proportionally to the time,—which he ascribed to the successively diminished resistance of the air,—but did not know how to determine the correct dependence of the space fallen through on the time. It was first at about the middle of the sixteenth century that the thought appeared that gravity continually communicates impulses to the falling body, and these impulses are added to the impressed force which is already present and which gradually decreases. This view was embraced by A. Piccolomini, J. C. Scaliger, and G. Benedetti. Already Leonardo remarked, quite by the way, that the arrow is not projected only at the greatest tension of the bow, but also in the other positions by the touching string (Duhem, *loc. cit.*, p. 882). But it was only when Galileo gave up this supposition of a gradual and spontaneous decrease of the impressed force and reduced this decrease to resisting forces, and investigated the motion of falling experimentally and without taking its causes into consideration, could the laws of the uniformly accelerated motion of falling appear in a purely quantitative form.

Further, from Duhem's historical exposition results the fact that Descartes rendered, independently of Galileo, more important services in the development of modern dynamics than is usually supposed, and than I too have supposed in the third

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chapter of my *Mechanics*. I am very grateful for this instruction. Descartes busied himself during his residence in Holland (1617–19), in co-operation with Beeckmann and in connection with the researches of Cardano and probably also of Scaliger and Benedetti, with the acceleration of falling bodies. He thoroughly recognised the law of inertia, as results from letters written to Mersenne in 1629, before Galileo's publication (E. Wohlwill, in *Die Entdeckung des Beharrungsgesetzes*, pp. 142, 143, considered it possible that Galileo indirectly stimulated him). Descartes also recognised the law of uniformly accelerated motion under the influence of a constant force, and was only mistaken with respect to the law of dependence of the path described on the time.

The thoughts of Galileo and Descartes mutually complete each other. Galileo investigated the motion of descent phenomenologically, and without inquiring into its causes, while Descartes derived this motion from the constant force. Naturally in both investigations a constructive and speculative element was active, but this element with Galileo kept close to the concrete case, while with Descartes it came in earlier with more general experiences. Certainly Descartes, in his *Principles of Philosophy*,[1]

[1] [This work was first published at Amsterdam in 1644 under the title: *Renati Des-Cartes Principia Philosophiæ*, and this was the only edition that appeared in Descartes' lifetime. A translation into French was made by one of Descartes' friends, the Abbé Claude Picot. Descartes

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observed the transference of motion and the loss of motion of the impinging body and the general philosophical consequences that (1) without the giving of motion to other bodies there can be no loss of motion (inertia); (2) every motion is either original or transferred from somewhere; (3) the original quantity of motion cannot be increased or diminished. From this standpoint he could imagine that every apparently spontaneous motion whose origin was not perceptible was introduced by invisible impacts.

The great advantage which I—perhaps in opposition to Duhem—ascribe to the method of Galileo consists in the careful and complete exposition of the mere facts. In this exposition nothing remains concealed behind the expression "force" which could be conjectured or disentangled by speculation. On this point opinions are divided even at the present time.

XI

[On p. 194, line 10, add:]

Baliani, in his preface to *De motu gravium* of 1638, distinguished, according to G. Vailati, between the weight as *agens* and the weight as *patiens*, and is therefore a forerunner of Newton.

read this translation and found it much to his taste, and, when it was completed in 1674, wrote a preface to it. This French translation passed through many editions; the fourth was published at Paris in 1681, and bears the title: *Les Principes de la Philosophie de René Descartes. Quatrième edition. Reveuë et corrigée fort exactement par Monsieur CLR.*]

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XII

[To p. 201, line 18, add :]

Newton's achievements are not limited to the domain which is the subject of this book. Even his *Principia* treats questions which do not belong to mechanics proper. Motion in resisting media and the motion of fluids—even under the influence of friction—are treated there, and the velocity of the propagation of sound is theoretically deduced for the first time. The optical works of Newton contain a series of the most important discoveries. He demonstrated the prismatic decomposition of light and the compounding of white light from rays of light of different colours and unequal refrangibilities, and, in this connection, gave a proof of the periodicity of light and determined the length of period as a function of the colour and refrangibility. Also it was Newton who first grasped the essential point in the polarisation of light. Other studies led him to establish his law of cooling and the thermometric or pyrometric principle founded on this law.[1] In his papers and book on optics[2] Newton showed the

[1] [*Cf.* Mach, *Die Principien der Wärmelehre*, 2nd ed., Leipsic, 1900, pp. 58–61.]

[2] [Newton's *Opticks: or a Treatise of the Reflexions, Refractions, Inflexions, and Colours of Light; also Treatises of the Species and Magnitude of Curvilinear Figures* was published at London in 1704, and again, with additions but without the mathematical appendices, in 1717, 1718, 1721, and 1730. A Latin translation, by Samuel Clarke, was first published at London in 1706; and a useful annotated German translation by W. Abendroth was published as Nos. 96 and 97 of

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paths which led to his discoveries quite frankly and without any restraint. Apparently the unpleasant controversies in which these first publications of his involved him had an influence on his exposition in the *Principia*. In the *Principia* he gave the proofs of the theorems that he had discovered in a synthetic form, and did not disclose the methods which had led him to these theorems. The acrimonious controversy between Newton and Leibniz, and between their respective followers, on the priority of the discovery of the infinitesimal calculus, was chiefly caused by the late publication of Newton's method of fluxions. To-day it is quite clear that both Newton and Leibniz were stimulated by their predecessors and had no need to borrow from one another, and also that the discoveries were sufficiently prepared for to enable them to appear in different forms. The preparatory works of Kepler, Galileo, Descartes, Fermat, Roberval, Cavalieri, Guldin, Wallis, and Barrow were accessible to both Newton and Leibniz.[1]

Ostwald's Klassiker der exakten Wissenschaften in 1898. Newton's *Optical Lectures read in the Publick Schools of the University of Cambridge Anno Domini, 1669*, was translated into English from the original Latin and published at London in 1728, after Newton's death. The Latin was published at London in 1729. Newton's papers on optics are printed in vols. vi–xi of the *Philosophical Transactions*, and begin in the year 1672.]

[1] [On Newton's mathematical and physical achievements, we may refer to M. Cantor's *Vorlesungen über Geschichte der Mathematik*, vol. iii, 2nd ed., Leipsic, 1901, pp. 156–328, and F. Rosenberger's excellent compilation, *Isaac Newton und seine physikalischen Principien*, Leipsic, 1895.]

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XIII

[On p. 539, end of Appendix XVII, add :]

It is remarkable that Galileo, in his theory of the tides, treated the *first* dynamical problem about the world without troubling about the new system of co-ordinates. He considered in the most naïve manner the fixed stars as the new system of reference.

XIV

[To p. 222, line 2, add :]

These sentences were contained in the first edition of 1883, and thus long before the discussion of electro-magnetic mass had begun.

I may here refer to A. Lampa's paper "Eine Ableitung des Massenbegriffs" in the Prague Journal *Lotos*, 1911, p. 303, and especially to the excellent remarks on the general method of treatment of such questions on pp. 306 *et seqq.*

XV

[On p. 225, instead of note, put :]

On the physiological nature of the sensations of time and space *cf. Analyse der Emfindungen*, 6th ed. ;[1] *Erkenntnis und Irrtum*, 2nd ed.

[1] [An English translation, published by the publishers of the present volume, of this edition under the title : *The Analysis of Sensations*, . . . in 1914.]

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XVI

On p. 542, end of Appendix XIX, omit the words "to which I shall reply in another place," and add the reference: *Erkenntnis und Irrtum*, 2nd ed., Leipsic, 1906, pp. 434–448.

XVII

[To p. 229, line 2, add:]

If, in a material spatial system, there are masses with different velocities, which can enter into mutual relations with one another, these masses present to us forces. We can only decide how great these forces are when we know the velocities to which those masses are to be brought. *Resting* masses too are forces if *all* the masses do not rest. Think, for example, of Newton's rotating bucket in which the water is not yet rotating. If the mass m has the velocity v_1 and it is to be brought to the velocity v_2, the force which is to be spent on it is $p = m(v_1 - v_2)/t$, or the work which is to be expended is $ps = m(v_1^2 - v_2^2)$. *All* masses and *all* velocities, and consequently *all* forces, are relative. There is no decision about relative and absolute which we can possibly meet, to which we are forced, or from which we can obtain any intellectual or other advantage. When quite modern authors let themselves be led astray by the Newtonian arguments which are derived from the bucket of water, to

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distinguish between relative and absolute motion, they do not reflect that the system of the world is only given *once* to us, and the Ptolemaic or Copernician view is *our* interpretation, but both are equally actual. Try to fix Newton's bucket and rotate the heaven of fixed stars and then prove the absence of centrifugal forces.

XVIII

[To p. 229, line 18, add :]

We must suppose that the change in the point of view from which the system of the world is regarded which was initiated by Copernicus, left deep traces in the thought of Galileo and Newton. But while Galileo, in his theory of the tides, quite naïvely chose the sphere of the fixed stars as the basis of a new system of co-ordinates, we see doubts expressed by Newton as to whether a given fixed star is at rest only apparently or really (*Principia*, 1687, p. 11). This appeared to him to cause the difficulty of distinguishing between true (absolute) and apparent (relative) motion. By this he was also impelled to set up the conception of *absolute space*. By further investigations in this direction — the discussion of the experiment of the rotating spheres which are connected together by a cord and that of the rotating water-bucket (pp. 9, 11)—he believed that he could prove an absolute rotation, though

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he could not prove any absolute translation. By absolute rotation he understood a rotation relative to the fixed stars, and here centrifugal forces can always be found. "But how we are to collect," says Newton in the Scholium at the end of the Definitions, "the true motions from their causes, effects, and apparent differences, and *vice versa*; how from the motions, either true or apparent, we may come to the knowledge of their causes and effects, shall be explained more at large in the following Tract." The resting sphere of fixed stars seems to have made a certain impression on Newton as well. The natural system of reference is for him that which has any uniform motion or translation without rotation (relatively to the sphere of fixed stars).[1] But do not the words quoted in inverted commas give the impression that Newton was glad to be able now to pass over to less precarious questions that could be tested by experience?

XIX

[Instead of line 4 up of p. 232 to line 18 of p. 233, put:]

When Newton examined the principles of mechanics discovered by Galileo, the great value of the simple and precise law of inertia for deductive

[1] *Principia*, p. 19, Coroll. V: "The motions of bodies included in a given space are the same among themselves, whether that space is at rest or moves uniformly forwards in a right line without any circular motion."

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derivations could not possibly escape him. He could not think of renouncing its help. But the law of inertia, referred in such a naïve way to the earth supposed to be at rest, could not be accepted by him. For, in Newton's case, the rotation of the earth was not a debatable point; it rotated without the least doubt. Galileo's happy discovery could only hold approximately for small times and spaces, during which the rotation did not come into question. Instead of that, Newton's conclusions about planetary motion, referred as they were to the fixed stars, appeared to conform to the law of inertia. Now, in order to have a generally valid system of reference, Newton ventured the fifth corollary of the *Principia* (p. 19 of the first edition). He imagined a momentary terrestrial system of co-ordinates, for which the law of inertia is valid, held fast in space without any rotation relatively to the fixed stars. Indeed he could, without interfering with its useability, impart to this system any initial position and any uniform translation relatively to the above momentary terrestrial system. The Newtonian laws of force are not altered thereby; only the initial positions and initial velocities—the constants of integration—may alter. By this view Newton gave the *exact* meaning of his hypothetical extension of Galileo's law of inertia. We see that the reduction to absolute space was by no means necessary, for the system of reference is just as relatively determined

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as in every other case. In spite of his metaphysical liking for the absolute, Newton was correctly led by the *tact of the natural investigator.* This is particularly to be noticed, since, in former editions of this book, it was not sufficiently emphasised. How far and how accurately the conjecture will hold good in future is of course undecided.

XX

[To p. 238, line 3, add :]

I do not believe that the writings of the advocates of absolute space which have appeared during the last ten years can assert anything else than the italicised passage, which stood in the first German edition of 1883 (pp. 221, 222).

XXI

[Appendix XX, on pp. 542–547, is, in the seventh German edition, partly omitted, and the following inserted :]

The law of inertia has often been discussed in ancient and modern times, and almost always the empty conception of absolute space, which is open to such grave objections in point of principle, has mixed itself up with it in a disturbing manner. Here we will limit ourselves to the mention of the more modern discussions of this subject.

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In the first place we must mention the writings of C. Neumann: *Ueber die Principien der Galilei-Newton'schen Theorie*, of 1870, and "Über den Körper Alpha" (*Ber. der königl. sächs. Ges. der Wiss.*, 1910, iii). The author denotes, on p. 22 of the former treatise, the relation to the body Alpha as a relation to a system of axes which proceeds uniformly in a straight line without rotation, and thus his statement coincides with the fifth corollary of Newton which we have already mentioned. However, I do not believe that the fiction of the body Alpha and the preservation of the distinction between absolute and relative motion and the paradoxes (pp. 27, 28) connected with this distinction have particularly contributed to the clarification of the matter. In the publication of 1910 (p. 70, note 1) Neumann calls what he has brought forward purely hypothetical, and in this lies an essential progress in the knowledge of Newton's fifth corollary. In the same publication, Lange's standpoint is exposed as in essentials coinciding with his own.

H. Streintz (*Die physikalischen Grundlagen der Mechanik*, 1883) accepts the Newtonian distinction between absolute and relative motion, but also comes to the view expressed in Newton's fifth corollary. What I had to say against Streintz's criticism of my views was contained in the former editions of this work and shall not be repeated here.

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We will now consider L. Lange: "Über die wissenschaftliche Fassung der Galilei'schen Beharrungsgesetzes," Wundt's *Philos. Studien*, vol. ii, 1885, pp. 266–297, 539–545; *Ber. d. königl. sächs. Ges. der Wiss., math.-physik. Klasse*, 1885, pp. 333–351; *Die geschichtliche Entwicklung des Bewegungsbegriffs*, Leipsic, 1886; *Das Inertialsystem vor dem Forum der Naturforschung*, Leipsic, 1902.

L. Lange sets out from the supposition that the general Newtonian law of inertia subsists and seeks the system of co-ordinates to which it is to be referred (1885). With respect to any moving point P_1—which can even move in a curve,—we can so move a system of co-ordinates that the point P_1 describes a straight line G_1 in this system. If we have also a second moving point P_2, the system can still be moved so that a second straight line G_2, in general warped with respect to G_1, is described by P_2, if only the shortest distance G_1 G_2 does not surpass the shortest distance which P_1 P_2 can ever have. Still the system can rotate about P_1 P_2. If we choose a third straight line G_3, such that all the triangles P_1, P_2, P_3 which can arise by means of any third moving point P_3 are representable by points on G_1, G_2, G_3, then P_3 can also advance on G_3. Thus, for at most three points, a system of co-ordinates in which these points proceed in a straight line is a mere convention. Now, Lange sees the essential contents of the law of inertia in that, by the help of three material

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points which are left to themselves, a system of co-ordinates can be found with respect to which four or arbitrarily many material points which are left to themselves move in a straight line and describe paths which are proportional to one another. The process in nature is thus a simplification and limitation of the kinematically possible variety of cases.

This promising thought and its consequences found much recognition with mathematicians, physicists, and astronomers. (*Cf.* H. Seeliger's account of Lange's works in the *Vierteljahrsschrift der astronom. Ges.*, vol. xxii, p. 252; H. Seeliger, "Über die sogenannte absolute Bewegung," *Sitzungsber. der Münchener Akad. der Wiss.*, 1906, p. 85.) Now, J. Petzoldt ("Die Gebiete der absoluten und der relativen Bewegung," Ostwald's *Annalen der Naturphilosophie*, vol. vii, 1908, pp. 29–62) has found certain difficulties in Lange's thoughts, and these difficulties have also disturbed others and are not quickly to be put on one side. On this account we will here break off our remarks on Lange's system of co-ordinates or inertial systems till the clouds pass away. Seeliger has attempted to determine the relation of the inertial system to the empirical astronomical system of co-ordinates which is in use, and believes that he can say that the empirical system cannot rotate about the inertial system by more than some seconds of arc in a century. *Cf.*

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also A. Anding, "Über Koordinaten und Zeit," in vol. vi of the *Encyklopädie der mathematischen Wissenschaften.*

The view that "absolute motion" is a conception which is devoid of content and cannot be used in science struck almost everybody as strange thirty years ago, but at the present time it is supported by many and worthy investigators. Some "relativists" are: Stallo, J. Thomson, Ludwig Lange, Love, Kleinpeter, J. G. MacGregor, Mansion, Petzoldt, Pearson. The number of relativists has very quickly grown, and the above list is certainly incomplete. Probably there will soon be no important supporter of the opposite view. But, if the inconceivable hypotheses of absolute space and absolute time cannot be accepted, the question arises: In what way can we give a comprehensible meaning to the law of inertia? MacGregor shows in an excellent paper (*Phil. Mag.*, vol. xxxvi, 1893, pp. 233–264),[1] which is very clearly written and shows great recognition of Lange's work, that there are two ways that we can take: (1) the historical and critical way, which considers anew the facts on which the law of inertia rests and which draws its limits of validity and finally considers a new formulation; (2) the supposition that the law of inertia in its old form teaches us the motions sufficiently,

[1] [This paper, "On the Hypotheses of Dynamics," was occasioned by some remarks of O. Lodge on a former paper of MacGregor's.]

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and the derivation of the correct system of co-ordinates *from* these motions.

For the first method it seems to me that Newton himself gave the first example with his system of reference indicated in the fifth corollary, which has been often mentioned above. It is obvious that we must take account of modifications of expression which have become necessary by extension of our experience. The second way is very closely connected *psychologically* with the great trust which mechanics, as the most exact natural science, enjoys. Indeed, this way has often been followed with more or less success. W. Thomson and P. G. Tait (*Treatise on Natural Philosophy*, vol. i, part 1, 1879, § 249)[1] remark that two material points which are simultaneously projected from the same place and then left to themselves move in such a way that the line joining them remains parallel to itself. Thus, if four points O, P, Q, and R are projected simultaneously from the same place and then subject to no further force, the lines OP, OQ, and OR always give fixed directions. J. Thomson attempts, in two articles (*Proc. Roy. Soc. Edinb.*, 1884, pp. 568, 730), to construct the system of reference corresponding to the law of inertia, and in this recognises that the suppositions about uniformity and rectilinearity are *partly conventional.* Tait (*loc. cit.*, p. 743), stimulated by J. Thomson, takes part

[1] [*Cf.* §§ 267, 245.]

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in the solution of the same problem by quaternions. We find also MacGregor in the same path ("The Fundamental Hypotheses of Abstract Dynamics," *Trans. Roy. Soc. of Canada*, vol. x, 1892, § iii, especially pp. 5 and 6).

The same psychological motives were certainly active in the case of Ludwig Lange, who has been most fortunate in his efforts correctly to interpret the Newtonian law of inertia. This he did in two articles in Wundt's *Philos. Studien* of 1885.

More recently Lange (*Philos. Studien*, vol. xx, 1902) published a critical paper in which he also worked out the method of obtaining a *new* system of co-ordinates according to his principles, when the usual rough reference to the fixed stars shall be, in consequence of more accurate astronomical observations, no longer sufficient. There is, I think, no difference of meaning between Lange and myself about the *theoretical* and formal value of Lange's expressions, and about the fact that, at the present time, the heaven of fixed stars is the only practically usable system of reference, and about the method of obtaining a new system of reference by gradual corrections. The difference which still subsists, and perhaps will always do so, lies in the fact that Lange approaches the question as a *mathematician*, while I was concerned with the *physical* side of the subject.

Lange supposes with some confidence that *his*

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expression would remain valid for celestial motions on a large scale. I cannot share this confidence. The surroundings in which we live, with their almost constant angles of direction to the fixed stars, appear to me to be an extremely special case, and I would not dare to conclude from this case to a very different one. Although I expect that astronomical observation will only as yet necessitate very small corrections, I consider it possible that the law of inertia in its simple Newtonian form has only, for us human beings, a meaning which depends on space and time. Allow me to make a more general remark. We measure time by the angle of rotation of the earth, but could measure it just as well by the angle of rotation of any other planet. But, on that account, we would not believe that the *temporal* course of all physical phenomena would have to be disturbed if the earth or the distant planet referred to should suddenly experience an abrupt variation of angular velocity. We consider the dependence as not immediate, and consequently the temporal orientation as *external*. Nobody would believe that the chance disturbance—say by an impact—of one body in a system of uninfluenced bodies which are left to themselves and move uniformly in a straight line, supposing that all the bodies combine to fix the system of co-ordinates, will immediately have a disturbance of the others as consequence. The orientation is external here

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also. Although we must be very thankful for this, especially when it is purified from meaninglessness, still the natural investigator must feel the need of further insight—of knowledge of the *immediate* connections, say, of the masses of the universe. There will hover before him as an ideal an insight into the principles of the whole matter, from which accelerated and inertial motions result in the *same* way. The progress from Kepler's discovery to Newton's law of gravitation, and the impetus given by this to the finding of a physical understanding of the attraction in the manner in which electrical actions at a distance have been treated, may here serve as a model. We must even give rein to the thought that the masses which we see, and by which we by chance orientate ourselves, are perhaps not those which are really decisive. On this account we must not underestimate even experimental ideas like those of Friedländer[1] and Föppl,[2] even if we do not yet see any immediate result from them. Although the investigator gropes with joy after what he can immediately reach, a glance from time to time into the depths of what is uninvestigated cannot hurt him.

A small elementary paper of J. R. Schütz ("Prinzip der absoluten Erhaltung der Energie,"

[1] B. and J. Friedländer, *Absolute und relative Bewegung*, Berlin, 1896.

[2] "Über einen Kreiselversuch zur Messung der Umdrehungsgeschwindigkeit der Erde," *Sitzungsber. der Münchener Akad.*, 1904, p. 5; "Über absolute und relative Bewegung," *ibid.*, 1904, p. 383.

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Göttinger Nachrichten, math.-physik. Klasse, 1897) shows, on simple examples, that Newton's laws can be obtained from the principle spoken of. The term "absolute" is only meant to express that the principle is to be freed from an indeterminateness and arbitrariness. If we imagine the principle applied to the central impact of elastic masses m_1 and m_2 in the form of points, of initial velocities u_1 and u_2 and final velocities v_1 and v_2, we have

$$m_1 u_1^2 + m_2 u_2^2 = m_1 v_1^2 + m_2 v_2^2.$$

We can calculate v_1 and v_2 from u_1 and u_2 if we suppose that the principle of energy holds for any velocity of translation c directed in the same sense as u and v. We then have

$$m_1(u_1+c)^2 + m_2(u_2+c)^2 = m_1(v_1+c)^2 + m_2(v_2+c)^2.$$

If we subtract the first equation from the second, we get the equation of the principle of reaction :

$$m_1 u_1 + m_2 u_2 = m_1 v_1 + m_2 v_2,$$

in which c has dropped out. From the first and third equation we can calculate v_1 and v_2. By an analogous treatment of the "absolute" principle of energy, we get Newton's equation of force for a mass-point, and finally the law of reaction, with its corollaries of the conservation of the quantity of motion and the conservation of the centre of gravity. The study of this paper is very much to be recommended, since even the conception of mass can be

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derived by the help of the principle of energy. *Cf.* the section on "Retrospect of the Development of Dynamics" in my *Mechanics.*

XXII

[To p. 242, line 6 up, add:]

What is pleonastic and tautological in Newton's propositions is psychologically comprehensible if we imagine an investigator who, setting out from his familiar ideas of statics, is in the act of establishing the fundamental propositions of dynamics. At one time force is in the focus of consideration as a pull or a pressure, and at another time as determinative of accelerations. When, on the one hand, he recognises, by the idea of a pressure which is common to all forces, that all forces also determine accelerations, then this twofold notion leads him, on the other hand, to a divided and far from unitary representation of the new fundamental propositions. *Cf.* *Erkenntnis und Irrtum*, 2nd ed., pp. 140, 315.

XXIII

[To p. 243, last line, add:]

The theorems *a* to *e* were given in my note "Über die Definition der Masse" in Carl's *Repertorium der Experimentalphysik*, vol. iv, 1868; reprinted in *Erhaltung der Arbeit*, 1872, 2nd ed.,

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Leipsic, 1909.[1] *Cf.* also Poincaré, *La Science et l'hypothèse*, Paris, pp. 110 *et seqq.*

XXIV

[To p. 245, "Tait."]

On p. 243 of the seventh German edition there is only mention of "W. Thomson (Lord Kelvin)."

XXV

[To p. 245, beginning of VIII, add :]

Dynamics has developed in an analogous way to statics. Different special cases of motions of bodies were observed, and people tried to put these observations in the form of rules. But just as little as, from the observation of a case of equilibrium of the inclined plane or the lever, can be derived a mathematically exact and generally valid rule for equilibrium — on account of the inaccuracy of measurement,—so little can the corresponding thing be done for cases of motion. Observation only leads, in the first place, to the conjecturing of laws of motion, which, in their special simplicity and accuracy, are presupposed as *hypotheses* in order to try whether the behaviour of bodies can be logically derived from these hypotheses. Only if these hypo-

[1] English translation by Philip E. B. Jourdain under the title *History and Root of the Principle of the Conservation of Energy*, Chicago and London, The Open Court Publishing Company, 1911. The reprint referred to is given on pp. 80–85 of this translation.

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theses have shown themselves to hold good in many simple and complicated cases, do we agree to keep them. Poincaré, in his *La science et l'hypothèse*, is, then, right in calling the fundamental propositions of mechanics *conventions* which might very well have fallen out otherwise.

XXVI

[On p. 247, line 17, insert :]

He believed that he could conclude from this the proportionality of the spaces fallen through with the squares of the times of falling (*Ediz. Nazionale*, vol. viii, pp. 373, 374).

XXVII

[To p. 248, line 10 up, add :]

In the second infinitesimal supposition of Galileo —of proportionality of the velocity to the time of falling—the triangular surfaces of Galileo's construction (fig. 87 of my *Mechanics*) represent, in a beautiful and intuitive way, the paths that are described. With the first supposition, on the other hand, the analogous triangles have no phoronomical signification, and on this account the integration was not successful.

XXVIII

[On p. 255, line 12, add :]

It has been shown that the present form of our science of mechanics rests on a historical accident.

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This is also shown in a very instructive way by the remarks of Lieut.-Col. Hartmann in his paper on the "Définition physique de la force" (*Congrès international de philosophie*, Geneva, 1905, p. 728), and in *L'enseignement mathématique*, Paris and Geneva, 1904, p. 425. The author shows the use of the usual conceptions of different ideas.

XXIX

To note on p. 555: "It should be added that a second edition of *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit* appeared at Leipsic in 1909, and, as already mentioned, an English translation, under the title *History and Root of the Principle of the Conservation of Energy*, was published at Chicago and London in 1911."

The same remark applies to the notes on pp. 494, 496, and 567, and the text on pp. 580 and 585.

XXX

The passage on p. 571, line 10 up of text, to p. 572, line 2, is omitted in the seventh German edition, and the following added: "It must here again be emphasised that Newton, in his fifth corollary, often quoted above, and which alone has scientific value, does *not* make absolute space his system of reference."

On p. 572, the last sentence in the text is omitted, and the sentence added: "But that the

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world is without influence must not be supposed in advance. In fact, Neumann's paradoxes only vanish when we give up absolute space and do not go beyond the fifth corollary."

XXXI

[To p. 302, after line 19, add :]

A. Schuster of Manchester has proved in a very beautiful way, in the London *Philosophical Transactions* for 1876 (vol. clxvi, p. 715), that the forces which set the radiometer of Crookes and Geissler in motion are *inner* forces. If we put the vanes of the radiometer into rotation by means of light, after we have suspended the glass cover bifilarly, this cover immediately shows a tendency to rotate in a sense contrary to the vanes. Schuster was able to measure the magnitude of the forces which here came into action.

V. Dvořák of Agram, the discoverer of the acoustic reaction-wheel, has, at my request, carried out analogous experiments with his reaction-wheel. If we put the resonator-wheel into acoustical rotation, its light cylindrical glass cover, which floated on water, fell at once into rotation in the opposite sense, and this latter rotation, when the wheel only goes on rotating by inertia, also immediately reverses its sense of rotation. My son, Ludwig Mach, has, at my wish, improvised upon the experiment

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with Dvořák's wheel by replacing the glass cover by a light paraffined paper cover which floated on water. When such a paper cover was suspended bifilarly, every acceleration of the wheel showed an increased tendency to rotation in the opposite sense, and every retardation a diminished tendency of this kind; and this was shown in a very striking manner. Dvořák's experiments are explained by those with the motor represented in fig. 152 of my *Mechanics*, and, in especial, by the experiment of fig. 153*a*. *Cf.* A. Haberditzl, "Über kontinuierliche akustische Rotation und deren Beziehung zum Flächenprinzip," *Sitzungsber. der Wiener Akademie, math.-naturwiss. Klasse*, May 9th, 1878.

XXXII

The passage "In spite . . . reasonable conjecture" on pp. 305–308 and note on p. 308 of the *Mechanics* is omitted in the seventh German edition, and the passage added: "According to Wohlwill's researches (*Zeitschrift für Völkerpsychologie*, vol. xv, 1884, p. 387), Marci emphatically cannot be regarded as having advanced dynamics in the direction taken by Galileo."

XXXIII

[To p. 364, line 21, add :]

The paper of Lipschitz ("Bemerkungen zu dem Prinzip des kleinsten Zwanges," *Journal für Math.*,

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vol. lxxxii, 1877, pp. 316 *et seqq.*) contains profound investigations on the principle of Gauss. Many elementary examples, on the other hand, are to be found in K. Hollefreund's *Anwendungen des Gauss'schen Prinzips vom kleinsten Zwange* (Berlin, 1897). On the principle here spoken of and allied principles, see *Ostwald's Klassiker*, No. 167: *Abhandlungen über die Prinzipien der Mechanik von Lagrange, Rodrigues, Jacobi und Gauss*, edited by Philip E. B. Jourdain (Leipsic, 1908). The notes of Jourdain on pp. 31–68 go beyond the needs of a first orientation, and this orientation is the object of the present elementary book.

What is said on pp. 363–364 of my *Mechanics* stands in need of completion. If the masses of the system have no velocity, the actual motions only enter in the sense of possible work, which is consistent with the conditions of the system (C. Neumann, *Ber. der kgl. sächs. Ges. der Wiss.*, vol. xliv, 1892, p. 184). But if the masses have velocities, which can even be directed against the impressed forces, then the motions which are determined by the velocities and forces are superposed (Boltzmann, *Ann. der Phys. und Chem.*, vol. lvii, 1896, p. 45), and Ostwald's maximum-principle (*Lehrbuch der allgem. Chemie*, vol. ii, part i, 1892, p. 37) is, according to Zemplén's excellent and universally comprehensible remark (*Ann. der Phys. und Chem.*, vol. x, 1903, p. 428), unsuitable for the description of

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mechanical events, because it does not take account of the *inertia* of the masses. However, it remains correct that the (virtual) works which are consistent with the conditions become actual. My text, which was drawn up before 1882, could not, of course, take account of the attempts to found an energetical mechanics of two years later. For the rest, I cannot value these attempts so little as some do. Even the old "classical" mechanics has not arrived at its present form without passing through analogous stages of error. In particular, Helm's view (*Die Energetik nach ihrer geschichtlichen Entwickelung*, Leipsic, 1898, pp. 205–252) can hardly be objected to. *Cf.* my exposition of the equal justification of the conceptions of work and force (*Ber. der Wiener Akad.*, December 1873), and also many passages of my *Mechanics*, particularly pp. 248 *et seqq.*

XXXIV

[On p. 575, line 6 up, insert :]

So much was already laid down in the first German edition of 1883. The objection of Helm (*Energetik*, p. 247), in so far as it concerns my own work, is hardly just.

XXXV

[To p. 481, line 1, add :]

Cf. my paper, "Die Leitgedanken meiner naturwissenschaftlichen Erkenntnislehre und ihre Auf-

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nahme durch die Zeitgenossen" (*Scientia: Rivista di Scienza*, vol. vii, 1910, No. 14, 2; or *Physikalische Zeitschrift*, 1910, pp. 599–606).

XXXVI

[To p. 504, end of § 1, add from *Mechanik*, p. 480:]

With these lines, which were written in 1883, compare Petzoldt's remarks on the striving after *stability* in intellectual life ("Maxima, Minima und Ökonomie," *Vierteljahrsschr. für wiss. Philosophie*, 1891).

XXXVII

[To p. 507, last line, add:]

CONCLUSION

At the beginning of this book, the view was expressed that the doctrines of mechanics have developed out of the collected experiences of handicraft by an intellectual process of refinement. In fact, if we consider the matter without prejudice, we see that the savage discoverers of bow and arrows, of the sling, and of the javelin, set up the most important law of modern dynamics—the law of inertia—long before it was misunderstood with thorough-going perversity by Aristotle and his learned commentators. And although first ancient machines for throwing projectiles and catapults and then modern firearms brought this law daily before

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our eyes, many centuries were needed before the correct theoretical idealisation was discovered by the genius of Galileo and Newton. It lay in exactly the opposite direction to that in which the great majority of human beings expected it to lie. Not the conservation, but the decrease of the velocity of projection was to be theoretically explained and justified.

The simple machines,—the five mechanical powers—as they are described by Hero of Alexandria in the work of which an Arabian translation came down to the Middle Ages, are without question a product of handicraft. If, now, a child busies himself with mechanical work with quite simple and primitive means,—as was the case with my son, Ludwig Mach—the dynamical sensations observed in this connection and the dynamical experiences obtained when adaptive motions are made, make a powerful and lasting impression. If we pay attention to these sensations, we come closer, intellectually speaking, to the instinctive origin of the machines. We understand why a long lever which gives back a less pressure is preferred, and why a hammer which is swung round to the nail can transfer more work or *vis viva* to it. We understand at once by experiment the transport of loads on rollers, and also how the wheel—the fixed roller—arose. The making of rollers must have gained a great technical importance and have led to the discovery of the turning-lathe. In possession of this, mankind easily

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discovered the wheel, the wheel and axle, and the pulley. But the primitive turning-lathe is the very ancient fire-drill of savages, which had a bow and cord, though of course this primitive lathe is only fitted for small objects. The Arabians still use it, and, up to quite recent times, it was almost universally in use with our watchmakers. The potter's wheel of the ancient Egyptians was also a kind of turning-lathe. Perhaps these forms served as models for the larger turning-lathe, whose discovery, as well as that of the plumb-line and theodolite, is ascribed to Theodorus of Samos. On it pillars of stone may well have been turned (532 B.C.). Not all pieces of knowledge find a like use; often they lie fallow for a long time. The ancient Egyptians had wheels on the war-chariots of the king. They actually transported their huge stone monuments, with brazen disregard of the work of men, on sledges. What did the labour of slaves taken as prisoners in war matter to them? The prisoners ought to be thankful that they were not, in the Assyrian manner, impaled, or at least blinded, but only, quite kindly, in comparison with that, used as beasts of burden. Even our noble precursors in civilisation—the Greeks—did not think very differently.

But if we suppose even the best will for progress, many discoveries remain hardly comprehensible. The ancient Egyptians were not acquainted with the screw. In the many plates of Rossellini's work no

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trace of it is to be found. The Greeks ascribed, on doubtful reports, its discovery to Archytas of Tarentum (about 390 B.C.). But with Archimedes (250 B.C.) and with Hero (100 B.C.) we find the screw in very many forms as something well known. Hero can easily say—and even in a way that can be understood by modern schoolmen: "the screw is a winding wedge." But whoever has not yet seen or handled a screw will not by this indication discover one. By analogy with the cases spoken of before, we must suppose that, when an object in the form of a screw—such as a twisted rope or a pair of wires twisted together for ornamental purposes or the spindle-ring of an old fire-drill which had been worn spirally by the cord—fell, by chance, into someone's hand, the thought of construction of a screw lay near to the sensation of the twisting of this thing in and out of the hand. At bottom it is chance observations in which the faulty adaptation of human beings to their surroundings expresses itself, and which, when they are once remarked, gives rise to a further adaptation.

My son vividly describes how, in an ethnographical museum, the dynamical experiences of his youth again vividly came to life; how they were awakened again by the perceptible traces of the work on the objects exhibited. May these experiences be used for the finding of a universal genetic technology, and perhaps, by the way, lead a little deeper into the understanding of the primitive history of mechanics.

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CORRECTIONS TO BE MADE IN THE *MECHANICS*

P. xi, lines 13–14; for "metaphysical" read "speculative."

P. 14; the last paragraph and figure are omitted in the seventh German edition.

P. 51, line 8; add to "Galileo" the words "had before this, in 1594."

P. 123, line 22; after "Guericke" add: "which were, in part, demonstrated as early as 1654."

P. 148, line 4 up; for "Leibnitzians" read "Leibnizians." Similarly pp. 250, 270.

P. 188, line 3; instead of "mean distances" read "major axes."

P. 206, line 8; for "constant" read "continual."

P. 271, line 19; for "Leibnitz" read "Leibniz." Similarly pp. 272, 274, 275, 276, 425, 426, 449, 454, 575.

P. 303, line 8; after "is" add "as Prof. Tumlirz did."

P. 335, line 16; for "1715" is put in the seventh German edition "1714."

P. 514, line 6 up; add to "*géometrie*" the words "of 1900."

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P. 517, line 2 up; for "9" read "6."

P. 523, end of Appendix VIII; add reference: "*Erkenntnis und Irrtum*, 2nd ed., Leipsic, 1906."

P. 566, line 8; for "Appelt" read "Apelt."

P. 568, line 14; after "Höfler" add "(*loc. cit.*, pp. 120–164)."

P. 569, line 7 up of text; for "412, 448" read "412–448."

P. 576, line 8; for "unique" read "specialised."

P. 580, line 12 up; for "1875" read "1872."

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NOTES ON
MACH'S *MECHANICS*

BY

PHILIP E. B. JOURDAIN, M.A.(CANTAB.).

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NOTES ON MACH'S *MECHANICS*

MACH'S *Mechanics* has become the standard work on the history and philosophy of mechanics, and the author's wish (*Mechanics*, p. xvi) that no changes shall be made in the original text of his book is binding not only for personal reasons but also because the book is now a classic. Still, careful historical and critical research has shown me that there are some errors in the book, and that the references often needed to be verified, completed, and supplemented by other references to more easily accessible editions or translations. I hope that the result of the rather laborious work of annotation undertaken in consequence, which has been approved of by Professor Mach himself, will make the book even more useful to teachers and students.

PHILIP E. B. JOURDAIN.

P. xv, line 13 up: "*Sciences.*"

The part on the laws of motion in this book (see Preface, pp. viii–ix) is *not* by Clifford, but by Karl Pearson.

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P. 11, line 4 up:

On the deductions of Archimedes and Galileo, *cf.* Mach, *Conservation of Energy*, pp. 65–67. This abbreviation will always be used for the title: *History and Root of the Principle of the Conservation of Energy*, translated from Mach's work: *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit* (Prague, 1872; 2nd ed., Leipsic, 1900), and annotated by Philip E. B. Jourdain (Chicago and London, 1911).

P. 13, line 9: "LAGRANGE."

This is merely Lagrange's account of Galileo's investigation, in the *Mécanique analytique* of 1788, p. 3 (*cf. Œuvres de Lagrange*, vol. xi, pp. 2–3). Archimedes had used a similar consideration to determine the centre of gravity of a magnitude composed of two parabolic surfaces (*De planorum æquilibriis*, book ii, prop. 1).

Lagrange told Delambre (*cf. Notice* by Delambre in *Œuvres de Lagrange*, vol. i, p. xi) that he wrote for Daviet de Foncenex, among other things, a new theory of the lever in the *third* part of a memoir by Foncenex in the *Miscellanea Taurinensia* for 1759. In the second volume (1760–61) of the *Miscellanea Taurinensia* (pp. 299–322) is a paper by Foncenex entitled: "Sur les principes fondamentaux de la Mécanique," in the *fourth* section ("Du Levier," pp. 319–322) of which is a

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deduction, by a method depending on the use of Taylor's series, which certainly seems very like Lagrange's work, of the law of the lever. As regards physical principles, it starts from the fact that two forces, each equal to $p/2$, have the same effect as one force p applied halfway between them. *Cf.* my paper on "The Ideas of the *Fonctions Analytiques* in Lagrange's Early Work" in the *Proceedings* of the International Congress of Mathematicians held at Cambridge in 1912.

P. 23, last line:

Guido Ubaldi, in 1577, applied the principle of moments to the theory of simple machines.

P. 24, line 3:

On this work of Stevinus's, *cf.* Mach, *Conservation of Energy*, pp. 21–23.

P. 33, line 23:

Lagrange (*Œuvres*, vol. xi, pp. 9–11) refers also, on the subject of equilibrium on the inclined plane, to Galileo (*Mécanique*, first published in French by Father Mersenne in 1634) and Roberval (*Traité de Mécanique*, printed in Mersenne's *Harmonie universelle*, 1636).

P. 39, line 7: "first."

The composition of motions was known, says Lagrange, to Aristotle (*cf.* some passages in his *Mechanical Questions*), was used in the description

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of curves by Archimedes, Nicomedes, and others of the ancients, and Roberval, and was used in mechanics by Galileo (prop. 2 of the fourth day of his *Dialogues*, and his *Delle scienza meccanica*; see *Mechanics*, pp. 154–155, 526), Descartes, Roberval, Mersenne, Wallis, and others.

On the objections to the deduction of the parallelogram of forces from that of motions, see A. Voss, *Encykl. der math. Wiss.*, iv. 1, pp. 43–44.

P. 36, line 12: "death."

Pierre Varignon's *Projet de la nouvelle mécanique* appeared in 1687, at Paris, and his *Nouvelle Mécanique* in 1725.

Newton "proved" (see Mach's *Mechanics*, p. 242) the theorem of the parallelogram of accelerations as a corollary of his second law of motion.

P. 36, line 15: "theorem."

Nouvelle Mécanique, Sect. i, Lemme xvi.

P. 36, note:

On the history of Varignon's and Lami's discoveries, see Lagrange, *Œuvres*, vol. xi, pp. 15–17.

P. 40, line 13: "Bernoulli."

This paper by Daniel Bernoulli bears the title: "Examen Principiorum Mechanicæ," and is printed in *Comment. Acad. Sci. Imp. Petrop.*, vol. i, 1726 (published in 1728), pp. 126–142. The proof was simplified by d'Alembert (*Opusc. math.*, vol. i, 1761;

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cf. vol. vi, 1773) and Aimé (*Journ. de Math.*, vol. i, 1836, p. 335). See Voss, *Encykl. der math. Wiss.*, iv, 1, p. 44, note 109 (pp. 44–46 also contain references to the work of Poisson and others in demonstrating the parallelogram of forces).

P. 47, line 8 up:

The English translation has here misprinted "Cauchy" for "Varignon."

P. 51, line 7:

On the principle of virtual velocities and its connection with the principle of the impossibility of a *perpetuum mobile*, see *Conservation of Energy*, pp. 31–32. Lagrange said that Guido Ubaldi is the discoverer of the principle of virtual displacements. *Cf.* also Voss, *Encykl. der math. Wiss.*, iv, 1, p. 66, note 180.

P. 52, line 13: Toricelli."

De motu gravium naturaliter descendentium, 1664.

P. 54, line 10: "work."

This conception (Galileo's "moment") is fundamental in Wallis's treatment of statics in his *Mechanica* of 1670 and 1671. See also the reference to Descartes in Lagrange's *Mécanique*.

P. 56, line 7 up: "1717."

This letter was dated January 26th, 1717, and was printed at the head of Section IX of Varignon's

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Nouvelle Mécanique (vol. ii, p. 174). "In every equilibrium," says Bernoulli, "of any forces, in whatever manner they are applied on one another, whether immediately or mediately, the sum of the positive energies will be equal to the sum of the negative energies taken positively."

P. 63, last line:

For this example, *cf.* Euler, *Hist de l'Acad. de Berlin*, 1751, p. 193.

P. 65, line 10 up: "zero."

Fourier ("Mémoire sur la statique," *Journ. de l'Éc. polyt.*, cah. v, 1798, pp. 20 *et seqq.*; *Œuvres*, vol. ii, pp. 475–521, in especial p. 488) first considered the case of the conditions in a statical problem being expressed by inequalities instead of equations. Apparently independently of Fourier and of one another, this case was considered in publications of Gauss (1829) and Ostrogradski (1838). *Cf.* Voss, *Encykl. der math. Wiss.*, iv. 1, pp. 73–75; and my notes in *Ostwald's Klassiker*, No. 167, pp. 59–60. In connection with the formulation of Gauss's principle for inequalities, see Voss, *loc. cit.*, pp, 85–87; *Ostwald's Klassiker*, No. 167, pp. 64–65.

P. 65, line 9 up: "*Mechanics.*"

In the *second* and later editions (*Œuvres*, vol. xi, pp. 22–26), not the edition of 1788.

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P. 68, line 19: "cases."

On the proofs of the principle of virtual displacements (or velocities) of Fourier (1798, and therefore *before* Lagrange's proof of 1811), Lagrange (1811 and 1813), Laplace, Ampère (1806), Poinsot (1806), and others, see Voss, *Encykl. der math. Wiss.*, iv, 1, pp. 67–73.

P. 68, line 25: "Academy."

On the papers of Maupertuis and Euler on this "law of rest," see the *Monist* for July 1912 (vol. xxii, pp. 416–417, 436–437, 441–444), or the reprint in my book: *The Principle of Least Action* (Chicago and London, 1913), pp. 3–4, 23–24, 28–31. See also the paper, referred to on p. 73 of the *Mechanics*, by the Marquis de Courtivron, entitled: "Recherches de Statique et de Dynamique, ou l'on donne un nouveau principe général pour la considération des corps animés par des forces variables, suivant une loi quelconque," *Histoire de l'Académie Royale des Sciences. Année* 1749. *Avec les Mémoires de Math. et de Phys. pour la même Année*, Paris, 1753, pp. 15–27 of the *Mémoires* (on pp. 177–179 of the *Histoire* there is a short açcount of this memoir). The title and enunciation of the principles were given in the *Mémoires* for 1748, published in 1752 (*cf. Mémoires* for 1747, published in 1752, p. 698). The principle is that of all the positions which a system of bodies animated by any

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forces and connected in any way takes successively, that where the system has the greatest Σmv^2, is the same as that in which it would have been necessary to put it in the first place in order that it might stay in equilibrium.

P. 76, line 8 up: "enunciated."

The actual quotation (from the second edition of the *Vorlesungen über Dynamik* in the *Supplement-band* of Jacobi's *Gesammelte Werke*, p. 15) is as follows. Jacobi is speaking of the transition from Lagrange's variational form of d'Alembert's principle for a system of independent masses to that for a system in which there are equations of condition, and the displacements of the co-ordinates are virtual. "The above extension of our symbolic equation to a system limited by conditions is, of course, not proved, but only historically asserted. To say this appears necessary, for, although Laplace did not prove this extension in his *Mécanique céleste* any more than I have done here, yet this remark of Laplace's has been considered to be a proof. Poinsot (*Journ. de Math.*, vol. iii, p. 244) wrote a paper against this opinion, and said very correctly that mathematicians are often deceived by the very long ways that they have traversed, and sometimes also by the very short ones. They are deceived if they finally come, by means of very lengthy calculations, to an identity, but hold it to be a theorem. An

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example of the other kind of deception is given by our case. To prove this extension is in no way my intention; we will regard it as a principle which it is not necessary to prove. This is the view of many mathematicians, in particular is it that of Gauss." And Clebsch added a note to say that that was probably a verbal communication to Jacobi.

P. 109, line 15:

It may here be mentioned that convenient German translations or reprints, as the case may be, of the fundamental memoirs of Green and Gauss (*cf.* p. 398 of the *Mechanics*) on the theory of potential are published in *Ostwald's Klassiker*, as Nos. 61 and 2 (edited by A. von Oettingen and A. Wangerin) respectively.

P. 110, line 3: "source."

Cf. Mechanics, pp. 395–402.

P. 118, line 4: "1672."

A convenient German translation with notes, by F. Dannemann, made No. 59 of *Ostwald's Klassiker*, under the title: *Neue "Magdeburgische" Versuche über den leeren Raum.*

P. 130, line 1: "bodies."

These investigations are contained in the discourses for the third and fourth day of Galileo's *Discorsi e dimostrazioni matematiche intorno a due*

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nuove scienze, Leyden, 1638, of which a convenient German translation, with notes, was given by A. J. von Oettingen in *Ostwald's Klassiker*, No. 24. The discourses for the first and second days are translated in No. 11 of the same collection; while No. 25 contains the appendix to the third and fourth day, and the fifth and sixth day. The title of an English translation is given on p. 20 above.

P. 130, line 11: "following."

Klassiker, No. 24, pp. 16–17.

P. 130, last line: "on."

See *Mechanics*, pp. 247–248.

P. 131, line 19: "correct."

Klassiker, No. 24, pp. 21 *et seqq.*

P. 132, line 22: "table."

Klassiker, No. 24, p. 24.

P. 133, line 3:

Cf. Conservation of Energy, pp. 23–28.

P. 136, line 3: "side."

Klassiker, No. 24, p. 19.

P. 141, line 9 up: "of gravity."

As Lagrange does in his *Mécanique* (*cf.*, *e.g.*, *Œuvres*, vol. xi, p. 239).

P. 158, line 2: "force."

Joseph Bertrand, in a note to his edition of

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Mécanique (*cf. Œuvres de Lagrange*, vol. xi, p. 238), remarked that a part of Galileo's *Dialogo sopra le due massimi sistemi del mondo* . . . (Florence, 1710, pp. 185 *et seqq.*) seemed, in spite of a grave error, to contain the germ of Huygens' discovery of centrifugal force.

Huygens communicated some theorems on centrifugal force to the Royal Society of London, in the form of an anagram, in 1669, and thirteen theorems on centrifugal force were given, without proof, in the fifth part of the *Horologium oscillatorium* of 1673. The proofs of these theorems were given in the *Tractatus de vi centrifuga*, published after Huygens' death in the *Opuscula postuma*, Leyden, 1703. Of this there is a convenient German translation, with notes by F. Hausdorff, in *Ostwald's Klassiker*, No. 138, pp. 35–67, 72–79. The *Horologium* is translated in No. 192 of this collection by A. Hecksher and A. von Oettingen.

P. 160, line 2:

Descartes, in his *Géométrie* of 1637, used equations instead of proportions, and, though Wallis introduced the practice of working with equations instead of proportions in mechanics, in his treatise *Mechanica: sive De Motu, Tractatus Geometricus*, London (parts i and ii, 1670; part iii, 1671), Newton still used proportions in his *Principia* of 1687. It seems that the language of proportions

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causes less difficulties to a beginner, because, for example, the proportion "the velocities are as the times" cannot be misunderstood so readily as the equation $v = gt$. A beginner would think that we were equating the velocity to the time multiplied by a certain constant, and most authors (*cf. Mechanics*, p. 269) encourage this misunderstanding by their sacrifice of accuracy to brevity. The truth is, of course, that "v" does not stand for the velocity but for the numerical measure, in terms of some unit of velocity; and so on.

P. 161, line 4 up (*cf.* p. 251):

The references are: Richer, *Recueil d'Observations faites en Plusieurs Voyages*, . . . Paris, 1693; Huygens, *Discours de la Cause de la Pesanteur*, 1690, p. 145. *Cf.* Todhunter, *A History of the Mathematical Theories of Attraction and the Figure of the Earth, from the Time of Newton to that of Laplace*, London, 1873, vol. i, pp. 29–30.

P. 167, line 13: "imaginable."

On the principle of similarity with Aristotle, Galileo, Newton, Joseph Bertrand (1847) in especial, and many others, see *Encykl. der math. Wiss.*, iv, 1, pp. 23 (A. Voss, 1901), 478–480 (P. Stäckel, 1908).

P. 173, line 3:

Cf. Conservation of Energy, pp. 28–30.

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P. 175, line 9:

Cf. Mach, *Conservation of Energy*, pp. 28–30. Some light is thrown on Huygens' principle by the following considerations. One of Galileo's fundamental equations takes the form (*cf.* pp. 269–270 of *Mechanics*)

$$\phi h = v^2/2.$$

If we introduce the conception of *mass*, we can say that Huygens generalised this into the form (*cf.* p. 178 of *Mechanics*)

$$\Sigma ph = \tfrac{1}{2}\Sigma mv^2 ;$$

and this again, when the forces are not necessarily constant nor the paths of the masses rectilinear (*cf. Mechanics*, pp. 276–277, 343–344, 350), becomes

$$\Sigma\int p \,.\, ds = \tfrac{1}{2}\Sigma m(v^2 - v_0^2),$$

or, if a force-function U exists,

$$U - U_0 = T - T_0,$$

in the usual notation. Now, this is a *first integral* of the equations of motion of a system of masses, and thus, if, as in the case of the problem of the centre of oscillation, there is only *one* degree of freedom, this integral gives the complete solution of the problem (*cf.* my *Least Action*, referred to above, pp. 69–76).

P. 177, last line:

For continuation, see *Mechanics*, p. 331.

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P. 187, line 7 up:

The observations of Tycho Brahe enabled his friend and pupil, Johann Kepler (1571–1630), to subject the planetary motions to a far more searching examination than had yet been attempted. Kepler first endeavoured to represent the planetary orbits by the hypothesis of uniform motion in circular orbits; but, in examining the orbit of Mars, he found the deviations from a circle too great to be owing to errors of observation. He therefore tried to fit in his observations with various other curves, and was led to the discovery that Mars revolved round the sun in an elliptical orbit, in one of the foci of which the sun was placed. By means of the same observations he found that the radius vector drawn from the sun to Mars describes equal areas in equal times. These two discoveries were extended to all the other planets of the system and were published at Prague in 1609 in his *Nova Astronomia seu Physica Cœlestia tradita Commentariis de Motibus Stellæ Martis.* In 1619 he published at Linz his *Harmonia Mundi*, which contained his third great discovery — that the squares of the periodic times of any two planets in the system are to one another as the cubes of their distances from the sun.

P. 188, line 11:

That the paths of the planets were to be explained by a force constantly deflecting the planets from

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the straight line which they tend to describe uniformly was contemplated, before Newton, by Wren, Hooke, and Halley (*cf.* Halley's letter to Newton of June 29th, 1686, printed in W. W. Rouse Ball's *Essay on Newton's "Principia,"* London and New York, 1893, p. 162). Hooke, indeed (*ibid.*, p. 151; and *cf.* for a fuller account, a paper in the *Monist* for July 1913, vol. xxiii, pp. 353–384), read before the Royal Society in 1666 a paper explaining the inflection of a direct motion into a curve by a "supervening attractive principle." All three seem to have been stopped by the mathematical difficulties of the problem. It may be noticed that Halley, presumably in much the same manner as that used by Newton (*cf. Mechanics*, p. 189), concluded that the centripetal force varied inversely as the square of the distance.

P. 189, line 11 up: "analysis."

Newton (*Principia*, book i, section viii) derived his law of attraction from Kepler's laws. John Bernoulli (*Mém. de l'Acad. de Paris*, 1710, p. 521; *Opera*, vol. i, p. 470) showed conversely that a central force reciprocally proportional to the square of the distance leads to a Kepler's motion in a conic section. *Cf.* P. Stäckel, *Encykl. der math. Wiss.*, iv, 1, 1908, p. 494.

P. 189, last line:

This achievement of the imagination was, it seems

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(see my article in the *Monist* for July 1913), also performed, before Newton, by Robert Hooke. What chiefly distinguishes Newton's work on the theory of universal gravitation from that of Hooke is the far greater mathematical ability of Newton. It was partly Newton's good luck that, at the time (1665–66) when he began his scientific career, everything was ripe for the formation of a mathematical method out of the infinitesimal and fluxional ideas then current; it is this formation that really seems to be the decisive factor in the Newtonian mechanics of the heavens. As for the Newtonian principles of mechanics, they seem to have grown up as a result of the Newtonian theory of astronomy. Thus the conception of *mass* as distinguished from *weight* and the third law of motion plainly had their origin in extra-terrestrial considerations. We can trace noteworthy approximations to the Newtonian standpoint with respect to both these questions with Wallis and Hooke (*cf.* my article just cited).

P. 191, line 17: "earth."

Newton had satisfied himself, as early as 1666, that the moon was kept in her orbit by a gravitational force towards the earth, and had begun to suspect that gravitation was a universal property of matter; but at that time he seems to have supposed that masses of sensible size could only behave to one another approximately as attracting *points* at

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very great distances from one another. But it was only in 1685 (*cf.* W. W. Rouse Ball, *An Essay on Newton's "Principia,"* London and New York, 1893, pp. 116, 157) that Newton discovered that a spherical mass attracts external masses as if the whole mass were collected at the centre (this forms Proposition 71 of section xii of the first book of the *Principia*). "No sooner," said J. W. L. Glaisher at the commemoration (1887) of the bicentenary of the publication of the *Principia* (see Ball, *op. cit.*, p. 61), "had Newton proved this superb theorem—and we know from his own words that he had no expectation of so beautiful a result till it emerged from his mathematical investigation—than all the mechanism of the universe at once lay spread before him. When he discovered the theorems that form the first three sections of Book I [of the *Principia*], when he gave them in his lectures of 1684, he was unaware that the sun and earth exerted their attractions as if they were but points. How different must these propositions have seemed to Newton's eyes when he realised that these results, which he had believed to be only approximately true when applied to the solar system, were really exact! Hitherto they had been true only in so far as he could regard the sun as a point compared to the distance of the planets, or the earth as a point compared to the distance of the moon,—a distance amounting to only about sixty times the earth's radius—but now they were mathe-

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matically true, excepting only for the slight deviation from a perfectly spherical form of the sun, earth, and planets. We can imagine the effect of this sudden transition from approximation to exactitude in stimulating Newton's mind to still greater efforts. It was now in his power to apply mathematical analysis with absolute precision to the actual problems of astronomy."

P. 192, line 7 up:

The *Principia* is reprinted in the second volume of the only complete edition of Newton's works, which was published in five volumes at London in 1779–85, under the title: *Isaaci Newtoni Opera quæ exstant omnia Commentariis illustrabat Samuel Horsley*. Further details about the editions and translations of this and other works of Newton are to be found in G. J. Gray's book: *A Bibliography of the Works of Sir Isaac Newton, together with a List of Books illustrating his Works*, Cambridge, 1907. The best-known English translation of the *Principia* is by Andrew Motte (*The Mathematical Principles of Natural Philosophy*, 2 volumes, London, 1729; American editions in one volume, New York, 1848 and 1850). This translation includes the preface which Roger Cotes prefixed to the second edition of 1713 of the *Principia*, which was edited by him in Latin. A third edition was edited by Henry Pemberton in 1726.

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P. 200, line 16:

It is remarkable that this passage in the *Principia* (Scholium to the Laws of Motion) was not referred to by Mach in his tract on the *Conservation of Energy* as showing that the principle of the excluded *perpetuum mobile* lies at the bottom of our instinctive perception of the truth of the third law. That this is so was recognised by J. B. Stallo (*The Concepts and Theories of Modern Physics*, fourth edition, London, 1900; *cf.* the references in my notes to Mach's *Conservation of Energy*, pp. 98, 99, 101), whose views on the part played in science of all ages by the principle of energy very closely resemble those of Mach. Otherwise the third law seems by no means to be the plain expression of an instinctive perception. By Hertz's evidence (*cf. Mechanics*, pp. 549–550) and the experience of some of us when being taught mechanics, we know that the point is often not grasped, and there is, in the traditional Newtonian form, no appeal to what instinctive knowledge we may possess. *Cf.* § xiv of my article in the *Monist* for October 1914 (vol. xxiv, pp. 553–555).

P. 201, line 18:

An account of Newton's achievements is also given by E. Dühring on pp. 172–211 of his *Kritische Geschichte der allgemeinen Principien der Mechanik*, 3rd ed., Leipsic, 1887. Dühring does not, however,

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criticise Newton's conception of *mass*. *Cf.* my article in the *Monist* for April and October 1914.

P. 216, line 8 up:

Rosenberger (*Isaac Newton und seine physikalischen Principien*, Leipsic, 1895, p. 173) has practically remarked that this is what Newton himself did. "From what Newton says further on, it appears that he supposed that all of the smallest particles of matter are equally dense and of the same size, and put the density proportional to the number of these particles in a given space." *Cf.* my article in the *Monist* for October 1914.

P. 238, line 5:

On the law of inertia see also *Conservation of Energy*, pp. 75–80.

P. 244, line 11: "convention."

Cf. Voss's (*Encykl. der math. Wiss.*, iv, 1, 1901, pp. 49–50) fundamental propositions of dynamics. The account, with many references (*ibid.*, pp. 50–56), of critical researches on the independence of Newton's axioms, the concept of mass, the principle of inertia, the conception of force, and the law of action and reaction should also be consulted.

P. 278, line 9, "*Gauss.*"

Gauss's paper on the reduction of the intensity of the force of terrestrial magnetism to absolute measure ("Intensitas vis magneticæ ad mensuram

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absolutam revocata," *Gött. Abh.*, 1832; *Werke*, vol. v, p. 81) was originally printed in Latin, and a convenient German edition, edited by E. Dorn, is published in No. 53 of *Ostwald's Klassiker.*

P. 278, line 17:

On the looseness of phrase according to which it is customary, in most treatises on mechanics and geometry, to talk about t, s, and v simply as the time, the distance, or the velocity, instead of the numerical measures of these quantities, see the above note to p. 160.

P. 288, line 9 up:

The law of the conservation of momentum was given by Newton in the third Corollary to his Laws of Motion, and that of the conservation of the centre of gravity in the fourth Corollary. Both are translated, together with the papers of Daniel Bernoulli and d'Arcy on the law of the conservation of areas (see p. 293 of *Mechanics*, and my note on it below), and that of Daniel Bernoulli, of 1748, on the principle of *vis viva* (see pp. 343, 348 of *Mechanics*, and my note to p. 343 below), in No. 191 of *Ostwald's Klassiker* (*Abhandlungen über jene Prinzipien der Mechanik, die Integrale der dynamischen Differentialgleichungen liefern, von Newton* (*1687*), *Daniel Bernoulli* (*1745*, *1748*) *und d'Arcy* (*1747*)), edited by Philip E. B. Jourdain.

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P. 293, last line:

The references are: Daniel Bernoulli, "Nouveau problème de mécanique résolu par . . .," *Hist. de l'Acad. de Berlin*, vol. i, 1745 (published 1746), pp. 54–70; Euler, "De Motu Corporum in superficiebus mobilibus," *L. Euleri Opuscula varii argumenti*, vol. i, Berlin, 1756, pp. 1–136 (*cf.* Dühring, *op. cit.*, pp. 285–286); Patrick d'Arcy, "Problème de Dynamique," *Hist. de l'Acad. Roy. des Sci.*, *1747* (Paris, 1752); *Mémoires*, pp. 344–361 (this consists of three memoirs read in 1743, 1746, and 1747 respectively; it is the second that contains the statement of the principle in question). On all these papers, see *Ostwald's Klassiker*, No. 191.

On d'Arcy's later statement (1749) of his principle of areas in a form which seemed to him to be preferable to that of Maupertuis' principle of least action, while answering the same purpose; and on the discussion arising from this between d'Arcy, Maupertuis, and Louis Bertrand, see *Monist*, July 1912, vol. xxii, pp. 445–456, or my *Least Action*, pp. 32–43.

P. 313, line 15: "theorems."

Wren admitted that he could not prove his theorems.

P. 313, line 17:

These experiments of Wallis, Wren, and Huygens are referred in Felix Hausdorff's notes to the German

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translation in No. 138 of *Ostwald's Klassiker*, of Huygens' posthumous treatises referred to on p. 314 (see the next note) of *Mechanics*. Wallis's treatise, referred to on p. 313 of *Mechanics*, is entitled *Mechanica: sive De Motu, Tractatus Geometricus*, and appeared at London in three parts: Parts I and II in 1670, and Part III, which contains the section "De Percussione," appeared in 1671. Of interest in this connection are the following extracts:—p. 4: "Per Pondus intelligo gravitatis mensuram"; p. 5: "Pondus sic intellectum, aut Gravitatis etiam, prout vel in Movente vel in Mobili, considerato; ita vel ad Movendi, vel ad Resistendi vim partinebit: Adeoque nunc ad Momentum, nunc ad Impedimentum referetur."

P. 314, line 7: "1703."

Huygens first published his laws of impact in a paper called "The Laws of Motion on the Collision of Bodies," in the *Philosophical Transactions* for 1669, and in "Regles du mouvement dans la rencontre des corps," in the *Journal des Savants*. But, a year earlier, Huygens spoke on these laws in the Paris Academy; and their discovery must date much further back, for, in a letter to Claude Mylon of July 6th, 1656, Huygens mentioned the increase of the action of impact by the interposition of an intermediate body. The extended derivation of the laws—the results of 1669 being given without

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proof—was given in the *Tractatus de motu corporum expercussione*, which appeared in Huygens' *Opuscula postuma*, edited by Burcherus de Volder and Bernhardus Fullenius (Lugduni Batavorum, 1703). A German translation of this *Tractatus*, with notes by F. Hausdorff, was given in 1903 in *Ostwald's Klassiker*, No. 138, pp. 3–34, 63–72.

P. 330, last line:

Further references on the subject of impulses are to be found in Voss's article in the *Encykl. der math. Wiss.*, iv, 1, 1901, pp. 56–58, 87–88.

P. 336, line 21:

The greater part of d'Alembert's *Traité de dynamique* was translated into German and annotated by Arthur Korn in No. 106 of *Ostwald's Klassiker*.

P. 343, line 12 up (*cf.* p. 348, line 4 up):

Daniel Bernoulli's paper, "Remarques sur le principe de la conservation des forces vives pris dans un sens général," was published in 1750 in the *Hist. de l'Acad. de Berlin* for 1748, pp. 356–364 (among the *Mémoires de la Classe de philosophie spéculative*). Jacobi (*op. cit.*, pp. 9–10) remarked that Daniel Bernoulli first noticed that one and the same U could serve for *all* the masses in the problem. In fact it is evident that partial differentiation with respect to the co-ordinate x only affects x, and consequently only those co-ordinates which multiply x

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enter the result. Bernoulli went, according to Jacobi, beyond Euler, and his point of view was developed by Lagrange.

P. 350, line 2 up of text: "233."

"Über ein neues allgemeines Grundgesetz der Mechanik," *Journ. für Math.*, vol. iv, 1829; *Werke*, vol. v, pp. 23–28; *Ostwald's Klassiker*, No. 167, pp. 27–30.

P. 352, line 10: "D'Alembert."

Gauss's principle is more general than d'Alembert's, in that it embraces cases where the conditions can only be expressed by equalities (*cf.* Voss, *Encykl. der math. Wiss.*, iv, 1, 1901, p. 86, note 229). Where the conditions are, as is usually the case, expressible by equations, Gauss's principle can, of course, be deduced from d'Alembert's. *Cf.* also *Ostwald's Klassiker*, No. 167, pp. 64–65, and, for another advantage of Gauss's principle, pp. 47–48.

P. 361, line 9 up: "principle."

On the analytical expression of Gauss's principle, see my notes in *Ostwald's Klassiker*, No. 167, pp. 47, 60–67. Here arises the interesting question, not dealt with by Mach, as to whether the Gaussian process of variation affects the co-ordinates and velocities, or only the accelerations. Mach's remark on p. 362, lines 5–8, of his *Mechanics* had been made by J. W. Gibbs ("On the Fundamental

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Formulæ of Dynamics," *Amer. Journ. of Math.*, vol. ii, 1879, pp. 49–64).

P. 364, line 7 up:

The early history of the principle of least action in Maupertuis's hands is described in detail in my paper in the *Monist* for July 1912, or my *Least Action*, pp. 1–46. The first account of Maupertuis's principles was given in a memoir read to the French Academy in 1744, and entitled "Accord de différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles" (*Histoire de l'Académie, Année 1744* (Paris, 1748), pp. 417–426), whereas Maupertuis's memoir "Les Loix du mouvement et du Repos déduites d'un Principe Métaphysique" was printed in the *Histoire de l'Académie de Berlin* for 1746, pp. 267–294. Mach's date of 1747 is thus a mistake: see the *Monist*, April 1912, vol. xxii, p. 285, or my *Least Action*, p. 47.

P. 367, line 13: "sense."

According to P. Stäckel (*Encykl. der math. Wiss.*, iv, 1, 1908, p. 491, note 125), Mach wrongly supposed that Maupertuis worked on the basis of the undulatory theory of light, whereas he really adopted the emission-theory, like a good Newtonian; and hence Mach mistakenly found a contradiction in Maupertuis's treatment.

Further, Maupertuis's principle *does* state that what $\int v \, . \, ds$ reduces to in this case is to be a

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minimum, and this was contested by Mach. However, *cf.* pp. 375–376 of his *Mechanics.*

On the subject of the principle of least action and the case of the motion of light, see *Monist,* April 1912, vol. xxii, pp. 285–288, and July 1912, vol. xxii, pp. 417–419; or my *Least Action,* pp. 47–50 and 4–6 respectively.

P. 368, line 16 up: "Euler's."

Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes: sive solutio problematis isoperimetrici latissimo sense accepti; Lausanne and Geneva, 1744; second appendix: *De motu projectorum in medio non resistente.* In the German translation of a part of the *Methodus* in *Ostwald's Klassiker,* No. 46 (among the classical works on the calculus of variations), this appendix does not appear.

The *Methodus* was published in the autumn of 1744, some months after Maupertuis's first paper on least action was presented to the French Academy; but, as A. Mayer (*Geschichte des Princips der kleinsten Action,* Akademische Antrittsvorlesung, Leipsic, 1877) pointed out, Euler's discovery was made under the stimulus of the Bernoullis, and independently of Maupertuis; but that later on Euler's own tendency towards metaphysical speculation combined with the influence of Maupertuis to make Euler treat his principle in a more general

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and *a priori*, and less precise way. *Cf.* my notes in *Ostwald's Klassiker*, No. 167, pp. 31–37; and my papers in the *Monist* for April (pp. 288–289) and July (pp. 429–445, 456–459) 1912; or my *Least Action*, pp. 50–51 and 16–32, 43–46 respectively.

P. 371, line 8: "holds."

This is not correct: Lagrange, in the memoir quoted below, in which he generalised (see *Mechanics*, p. 380) Euler's theorem, drew attention to the fact that the principle of least action does not depend for its validity on the principle of *vis viva*, which only follows from the equations of mechanics under the special condition (see *Mechanics*, p. 478) that the connections do not depend on the time. See my papers in the *Monist* for April (pp. 289–292) and July (pp. 456–457) 1912; or my *Least Action*, pp. 51–54 and 43–44 respectively.

The title of Lagrange's memoir is "Application de la méthode exposée dans le mémoire précédent a la solution de différents problèmes de dynamique," *Miscellanea Taurinensia* for 1760 and 1761 [published 1762], vol. ii, pp. 196–298; *Œuvres*, vol. i, pp. 365–468. This memoir immediately followed Lagrange's first fundamental memoir (see *Mechanics*, p. 436) on the calculus of variations: "Essai d'une nouvelle méthode pour déterminer les maxima et les minima des formules intégrales indefinies," *Misc. Taur.*, 1760 and 1761, vol. ii, pp. 173–195; *Œuvres*,

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vol. i, pp. 335–362; *Ostwald's Klassiker*, No. 47, pp. 3–30.

P. 371, line 8: "Jacobi."

In his "Vorlesungen über Dynamik," 2nd ed., in *Werke*, Supplementband, Berlin, 1884, pp. 45–49; *Ostwald's Klassiker*, No. 167, pp. 18–22, 58. Jacobi's sixth lecture and a part of the seventh, which refer to his (narrower than Lagrange's, as we know now) formulation of the principle of least action, were reprinted in *Ostwald's Klassiker*, No. 167, pp. 16–26.

The question of the relative generality of Euler's, Lagrange's, Hamilton's, and Jacobi's principles is discussed in my paper in the *Monist* for April 1912, vol. xxii, pp. 290–296, or my *Least Action*, pp. 51–58.

P. 372, line 10: "stead."

On these analogies, *cf. Mechanics*, pp. 425–427; and P. Stäckel, *Encykl. der math. Wiss.*, iv, 1, 1908, pp. 489–493.

P. 380, line 12 up:

This was first done in the *Miscellanea Taurinensia* for 1760 and 1761; see the *Monist* for April 1912, vol. xxii, pp. 289–291; or my *Least Action*, pp. 51–53.

P. 381, line 8:

On the opinions of Michel Ostrogradski, Adolf

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Mayer (1877), Helmholtz, and Réthy, that Hamilton's principle is a form of the principle of least action, and the clear distinction between these two principles in the work of Adolf Mayer (1886) and Otto Hölder, see the *Monist* for April 1912, vol. xxii, pp. 294–296, 301–303; or my *Least Action*, pp. 55–58, 63–65.

P. 390, note:

This memoir of Gauss's of 1829 is translated into German by Rudolf H. Weber, and annotated by H. Weber, in No. 135 of *Ostwald's Klassiker.*

P. 395:

The works cited on this page are: Newton, *Principia*, book iii, prop. 19; Huygens, "Dissertatio de causa gravitatis" (first published in French in 1690), *Opera posthuma*, vol. ii, p. 116; Bouguer, "Comparison des deux Loix que la Terre et les autres Planètes doivent observer dans la figure que la pesanteur leur fait prendre," *Mém. de l'Acad. des Sci. de Paris*, 1734, pp. 21–40; Clairaut, *Théorie de la Figure de la Terre, tirée des Principes de l'Hydrostatique*, Paris, 1743 (German translation by A. von Oettingen, annotated by Philip E. B. Jourdain, in No. 189 of *Ostwald's Klassiker.*)

A very conscientious and detailed report on the work of many of Clairaut's predecessors and followers is given in the two volumes of Isaac Todhunter's work: *A History of the Mathematical Theories of*

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Attraction and the Figure of the Earth, from the Time of Newton to that of Laplace (London, 1873).

P. 397, line 14:

The conception of the pressure (p) at any point of a fluid was introduced by Euler in his memoir in the *Histoire de l'Acad. de Berlin*, 1755, pp. 217–273, and, according to Todhunter (*op. cit.*, vol. i, pp. 26, 193), this introduction is the most important progress made in hydrostatics since Clairaut.

P. 398, line 19: "Gauss."

Convenient German editions, with notes by A. J. von Oettingen and A. Wangerin, of the fundamental works on the theory of the potential of Green (1828) and Gauss (1840), were published in Nos. 61 and 2, respectively, of *Ostwald's Klassiker.*

P. 420, last line:

Other authors on hydrodynamics are: d'Alembert (the end of his *Traité de Dynamique* of 1743; his *Traité des Fluides*, Paris, 1744; and his *Essai d'une nouvelle théorie sur la résistance des Fluides*, Paris, 1752) and Euler (*Hist. de l'Acad. de Berlin*, 1755).

P. 425, line 15:

At this point we may refer to the researches of Maupertuis on the principle of least action in the case of the motion of light: see the above note to p. 367 of *Mechanics*.

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P. 425, line 27:

The original proposal and solution of this problem of John Bernoulli's are given in a German translation by P. Stäckel, in No. 46 of *Ostwald's Klassiker.*

P. 426, line 7: "light."

On the analogies between the motion of masses, the equilibrium of strings, and the motion of light, see *Mechanics*, pp. 372–380.

P. 437, line 17: "say."

This supposition does not seem to be correct. For Lagrange (*cf. Œuvres*, vol. i, pp. 337, 345; *Ostwald's Klassiker*, No. 47, pp. 5, 13) expressly "varied" the independent variable, and, partly on this ground, held his method to be more general than Euler's. Thus, if he had expressed the action-integral in the form $\int 2T \, . \, dt$, he would have made the t to be affected by the δ. In Lagrange's development of the principle of least action, the question as to whether t should be varied or not did not come up, as δv was at once eliminated. But the question is one of great interest, and gave rise to many important works of Rodrigues, Jacobi, Ostrogradski, Mayer, and others: see *Ostwald's Klassiker*, No. 167, especially pp. 50–51, 56–58; and my paper in the *Monist* for April 1912, vol. xxii, pp. 292–295; or my *Least Action*, pp. 55–58 (where, too, the conceptions of the nature of a variation of

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Euler, Lagrange, Lacroix, Jacobi, Strauch, M. Ohm, Cauchy, and Stegmann are dealt with).

If t is to be varied, we must regard it, according to the conception of a "variation" derived from Jellett, in the work cited in *Mechanics*, p. 437, as a function of another variable, θ, so that $\delta\theta=0$, but δt is not zero in general. This was done explicitly by Helmholtz ("Zur Geschichte des Princips der kleinsten Aktion," *Sitzungsber. der Berliner Akad.*, Sitzung vom 10. März 1887, pp. 225–236; *Wissenschaftliche Abhandlungen*, vol. iii, pp. 249–263).

P. 437, line 7 up of text: "form."

Jellett (*op. cit.*, p. 1) defines this as "the nature of the relation subsisting between the dependent variable and the independent variables."

P. 438, line 15: "required."

Jellett (*op. cit.*, p. 2) denoted this function of a function by $F.\phi$, and assumed (*ibid.*, p. 4) that $F.\phi+F\phi_1=F(\phi+\phi_1)$. In the calculus of variations, F is d or $\int$, and the distributive law is verified for them. *Cf. ibid.*, pp. 11, 355.

P. 440, line 10:

A mere *plus* is used in the expression for DU, because (Jellet, *op. cit.*, p. 5) the theorem is perfectly analogous to the principle of the superposition of small motions in mechanics, and may be proved in a similar manner.

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P. 445, line 13: "function."

Jellett (*op. cit.*, pp. 311–313) drew a distinction between a *mechanical* variation, which is not a variation but a displacement, and a geometrical or mathematical one. In the former, "δ no longer denotes the increment which is produced by a change in position, by the motion of a particle from one point of space to another." The rules governing mechanical variations are generally the same as the rules in the calculus of variations.

P. 445, last line:

This remark is not quite correct. At the beginning of his career (1759), Lagrange announced his intention of deriving the whole of mechanics from the principle of the least quantity of action. He fulfilled this promise in a long memoir of 1760 and 1761, and it was only in 1764 that the equations of mechanics appeared in a form which was not the equating to zero of the variation of an integral (see *Monist*, April 1912, vol. xxii, pp. 289–293; or my *Least Action*, pp. 51–54). From after this early work of Lagrange until the time of Gauss, no predilection for expressing mechanical principles in a maximal or minimal form appeared, and the attempt of Gauss was hardly in a direction that would be approved of by Lagrange, who, as time went on, became very "anti-metaphysical" (*cf. Mechanics*, p. 457).

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Mach here only considers the calculus of variations for functions of *one* variable. The case of many variables is important in mechanics, and one of Lagrange's great advances was to consider this case. Jellett (*op. cit.*, pp. 18–27, 107–111) dealt with many variables which may be connected by equations of condition. If the functions y, z, are independent of each other, their variations will also be independent and arbitrary, and the coefficients of δy and δz under the integral sign are each to be equated to zero.

Jellett dealt with Lagrange's method of multipliers (see *Mechanics*, pp. 471–472) on pp. 20–23, 115–134, of his above quoted work, with non-integrable, in general, equations of condition.

P. 454, last line:

Mach appears to give Voltaire's version of some of the things dealt with by Maupertuis; but Maupertuis does not seem, by his published writings, to have been nearly so ridiculous a person as Voltaire, for personal reasons, tried to make him appear to be; see *Monist*, July 1912, vol. xxii, pp. 427–428; or my *Least Action*, pp. 14–15.

P. 466, line 8 up: "perspicuity."

Cf. Th. Körner, "Der Begriff des materiellen Punktes in der Mechanik der 18. Jahrhunderts," *Bibl. Math.* (3), vol. v, 1904, p. 15. Euler (*Mechanica sive motus scientia analytice exposita*,

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two vols., St Petersburg, 1736) and d'Alembert (*Traité de dynamique*, Paris, 1743; annotated German translation by A. Korn of most of it in *Ostwald's Klassiker*, No. 106) everywhere used "natural co-ordinates"; and the methodical introduction here of Cartesian co-ordinates is due to Maclaurin (*A Complete System of Fluxions*, Edinburgh, 1742, arts. 465, 469, 884). *Cf. Mechanics*, p. 466.

P. 466, line 5 up: "1788."

The various editions of Lagrange's *Mécanique* are as follows: (1) *Mécanique analitique*, Paris, 1788, in one volume; (2) *Mécanique analytique*, Paris, vol. i, 1811; vol. ii (posthumous), 1815; (3) *Mécanique analytique*, 2 vols. Paris, 1853 and 1855, with notes by Joseph Bertrand; (4) like the third edition, but with some additional notes by Gaston Darboux, in *Œuvres de Lagrange*, vols. xi and xii, Paris, 1888 and 1889.

D'Alembert's principle, in combination with the principle of virtual displacements, appeared in variational form (*cf. Mechanics*, pp. 342–343, 468) for the first time in a prize essay of Lagrange's of 1764 on the libration of the moon (*Œuvres*, vol. vi, pp. 5–61); and then, more fully, in a memoir of 1780 (*Œuvres*, vol. v, pp. 5–122) on the same subject.

It is well known that Lagrange founded the calculus of the differential quotients of functions on

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the seeking of the terms of the development of these functions by Taylor's series, and thus avoided the difficulties connected with infinitesimals. In 1797, nine years after the publication of the *Mécanique*, he had published a systematic exposition of this theory,—the *Théorie des fonctions analytiques*—with applications to mechanics. However, in the preface to the second edition (1811) of the *Mécanique*, Lagrange (*cf.* *Œuvres*, vol. xi, p. xiv) remarked: "I have kept the ordinary notation of the differential calculus, because it corresponds to the system of infinitesimals adopted in this treatise. When we have well conceived the spirit of this system, and when we have convinced ourselves of the exactitude of its results by the geometrical method of prime and ultimate ratios or by the analytical method of derived functions, we can use infinitesimals as a sure and convenient instrument for shortening and simplifying proofs. It is in this manner that we shorten the proofs of the ancients by the method of indivisibles."

P. 480, line 3 up:

Cf. on this point, notes of mine on pp. 106, 108, in *Conservation of Energy*.

P. 493, line 9:

This refers to *Conservation of Energy*, pp. 51–53, 86–88; *cf.* pp. 94, 97–98.

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P. 499, line 7:

Helmholtz's famous memoir of 1847 is reproduced, together with own notes of 1881, in No. 1 of *Ostwald's Klassiker*.

P. 502, line 19:

On this and what follows, *cf. Conservation of Energy*, pp. 61–64, 69–74, 98–102.

P. 510, line 5:

The anecdote of Newton and the falling apple rests on good authority. It was stated to be the fact by Conduitt, the husband of Newton's favourite niece, and was repeated later by Mrs Conduitt to Voltaire, through whom it became well known. It was also mentioned by others, and is confirmed by a local tradition. See Rouse Ball, *op. cit.*, pp. 11–12; Rosenberger, *op. cit.*, pp. 119–120. *Cf.* my article in the *Monist* for April 1914, vol. xxiv, p. 202.

P. 527, line 8 up: "Hamilton's."

Proc. of the Royal Irish Acad., March 1847 (read 1846); *Lectures on Quaternions*, Dublin, 1853, p. 614; *Elements of Quaternions*, London, 1866, pp. 100, 718 (a second edition of the *Elements* was published at London, 1899–1901). The concept of Hodograph was formed by Möbius as early as 1843 (*cf.* his *Mechanik des Himmels*; *Werke*, vol. iv, Leipsic, 1887, pp. 36, 47).

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P. 530, line 4:

It is to be remembered that Newton himself, although he rejected the undulatory theory of light because he did not think that this theory could explain the rectilinear propagation of light, considered the whole of space to be filled with an elastic medium which propagates vibrations in a manner analogous to that in which the air propagates vibrations of sounds. The æther penetrates into the pores of all material bodies whose cohesion it brings about; it transmits gravitational action, and its irregular turbulence constitutes heat. *Cf.* E. T. Whittaker, *A History of the Theories of Æther and Electricity from the Age of Descartes to the Close of the Nineteenth Century*, London and Dublin, 1910. *Cf.* also p. 534 of the *Mechanics*, and my articles in the *Monist* for April 1914 (vol. xxiv, pp. 219–223) and January and April 1915.

P. 532, line 3:

On these investigations of Hooke's, see my paper in the *Monist* for July 1913.

P. 554, line 9:

On this and other examples see L. Boltzmann, "Eine Anfrage betreffend ein Beispiel zu Hertz' Mechanik," *Jahresber. der deutsch. math.-Ver.*, vol. vii, 1899, pp. 76–77.

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P. 555, line 13:

This article is translated on pp. 80–85 of *Conservation of Energy.*

P. 563, line 13:

This is on pp. 27–28 of *Conservation of Energy.*

P. 567, second note:

See pp. 75–80, 105, of *Conservation of Energy.*

P. 578, line 9:

On Grassmann's ideas, *cf.* the references given in the above note to p. 480 of *Mechanics.*

P. 580, line 6 up:

See *Conservation of Energy*, p. 88.

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The Principle of Least Action. By PHILIP E. B. JOURDAIN. Pages 83. Stiff covers. Price 1s. 6d. net.

This historical and critical study consists of three parts. In the first part a very thorough study, from the original sources, of the work of Maupertuis, Euler, Daniel Bernoulli, König, d'Arcy, Louis Bertrand, and others, is made, and errors of Adolf Mayer, Mach, and Lord Morley corrected. In the second part the development of views on the principle is traced—again in great detail—through Lagrange, Rodrigues, Jacobi, Ostrogradski, and Hertz, up to the modern work of Hölder, Voss, Réthy, and the author, which enables us to solve the problem discussed in the last part, on "The Nature and Validity of the Principle of Least Action." Here also the early memoirs are subjected to criticism, and the outcome is not in all respects consistent with the traditional view, which view seems partly due to mistakes that can only be rectified by thorough historical research.

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"The discussion of the principle in question involved controversy doubly or trebly acute, because not only mathematics but metaphysical theology and questions of priority were involved. Mr Jourdain refers to every writer on the subject, and his work will be indispensable to subsequent inquirers."—*Mathematical Gazette.*

Man a Machine. By JULIEN OFFRAY DE LA METTRIE. Including Frederick the Great's Eulogy on La Mettrie and Extracts from La Mettrie's "Natural History of the Soul." Translated, with Notes, by GERTRUDE CARMAN BUSSEY. French-English edition. With a portrait of La Mettrie. Pages 226. Cloth, 6s. net.

La Mettrie was the most extreme writer among the earliest French materialists. His doctrine is an extension to man of Descartes' doctrine that animals are automata. Both the French corrected original, which is written in a lively and modern way, and English translation, and an essay on the philosophical position of La Mettrie with respect to earlier and subsequent writers, are included.

Problems of Science. By FEDERIGO ENRIQUES. Authorised translation by KATHARINE ROYCE, with an Introduction by JOSIAH ROYCE. Pages xvi, 392. Cloth, 10s. net.

The volume here translated is a comprehensive and compendious survey of what has been done and is yet to be done in the scientific field. The six chapters deal with the so-called unsolved problems: the squaring of the circle and perpetual motion; the various phases of illusion and reality, different kinds of facts which provide the data for the several sciences, methods of acquiring knowledge and different degrees attained; the problems of logic, first pure and then applied; the principles of geometry and the relation of geometry to physics; a discussion of the problems of time in its various aspects and also force statics, dynamics, mass, inertia; and lastly, the extension of mechanics, including the most recent discussion of the theory of relativity.

This book is, as the *Mathematical Gazette* says, useful to those interested either in philosophy or teaching.

"The most valuable portion of the work is the masterly analyses of the fundamental ideas used in the mathematical and mechanical sciences. . . . It is for this that we would commend Professor Enriques's *Problems of Science* to all philosophical students interested in epistomology."—*Oxford Magazine.*

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FOURTH THOUSAND OF MR BERTRAND RUSSELL'S "LOWELL LECTURES" OF 1914.

Our Knowledge of the External World as a Field for Scientific Method in Philosophy. By BERTRAND RUSSELL, M.A., F.R.S. Pages x, 245. Cloth, 7s. 6d. net.

These eight "Lowell Lectures," delivered at Boston (Mass.) in March and April 1914, attempt to show, by means of examples, the nature, capacity, and limitations of the logico-analytical method in philosophy, which, in the author's opinion, yields whatever scientific knowledge it is possible to obtain. It is because these lectures give the first published indication of the new investigations to which the logical and analytical school, of which Mr Russell is the most prominent exponent, is now tending, that they must be read and studied by all who are interested in science or philosophy, or scientific philosophy. They are on "Current Tendencies," "Logic as the Essence of Philosophy," "Our Knowledge of the External World," "The World of Physics and the World of Sense," "The Theory of Continuity," "The Problem of Infinity considered Historically," "The Positive Theory of Infinity," and "The Notion of Cause, with Applications to the Free-Will Problem." These lectures are written, as the *Mathematical Gazette* says, with that clearness, force, and subtle humour that readers of Mr Russell's other works have learnt to expect.

"*The* book of the year. . . . It is in every sense an epoch-making book."—*Cambridge Magazine.*

"This brilliant, lucid, amusing book, which, in spite of a few stift passages, everyone can understand."—*The New Statesman.*

"His method interests by the success with which it approximates philosophy to science. . . . These able and suggestive lectures will introduce thoughtful readers to a tract of speculative inquiry not yet much opened up, which promises good results to men with philosophic interests and scientific training."—*Scotsman.*

"The author maintains the fresh and brilliant yet easy style which always makes his writings a pleasure to read."—*Nature.*

"In some respects the most important contribution that has been made to philosophy for a long time past. The whole book is of extreme interest, and it abounds in good sayings."—*The International Journal of Ethics.*

Other reprints of classics on, and original works on sciences, philosophies, and religions are dealt with in the Open Court Company's Catalogue (sent post free). The Company also publishes a splendid series of portraits of philosophers and scientific men, and two magazines: *The Open Court* (6d. per copy, 5s. 6d. per year, post free), and *The Monist* (2s. 6d. per copy, 9s. 6d. per year, post free), both of which contain articles by leading men of philosophical, scientific, or religious interest.

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and fruitful path of investigation that we would otherwise have overlooked. And again, even the errors—the false and untenable hypotheses and so on—in the older works stand out more clearly to the student of to-day, and both prompt reflection on modern theories and afford a valuable training in sharpness of sight for the detection of such errors. This is perhaps the most valuable aid that history can give to research.

But we must not neglect the fact that—probably because we have been brought up in surroundings which are almost wholly due to the conquests made by science and philosophy, and possibly because we ourselves are so formed physiologically as to be better able to carry out scientific thinking than men of past generations—some considerations which were of the greatest importance in the past are comparatively unimportant at the present time. This is the case with very many experimental researches. We have become so used to thinking in terms of certain refinements of apparatus, and this apparatus is so easily obtainable, that the description of cruder apparatus is often not only wearisome but really of no advantage. Of course, this does not apply to the deeply interesting means whereby, for example, Galileo measured time without the use of those clocks which were only made possible by his dynamical results.

When we have once arrived at a scientific proof, the way in which other people arrived at it loses much of its importance. We may, and probably do, feel a great interest in such questions, but the fact that the connections of truths are independent of how people recognise them is strongly brought home to us. In a science we are primarily concerned with the truths about its subject-matter; not with the history of how people discovered some of these truths. History itself is a science, but in history we are not primarily concerned with historians and how they wrote their histories. But though there is no necessary connection between the fact that A

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implies B and the fact that some person C found, by the process D, that A implies B, the knowledge of the fact last named seems to be very often indispensable for us human beings in rising to the discovery by ourselves of the thing C discovered. The history of a science has a heuristic value for the student of that science. This heuristic value is partly due to the stimulus afforded by, and suggestions contained in, the original works of the masters—and this stimulus and suggestiveness hardly every finds its way into text-books. Secondly, there are often unworked mines of definite suggestion—by analogy, for example—for new researches in the original memoirs of the founders of science. Thirdly, science often returns in its own progress to older points of view. Fourthly, historical studies have been shown, by many brilliant works, to afford a valuable basis for criticism—and criticism that is important for new transformations of science. Lastly, students will be relieved and stimulated to find that the great discoverers at first made mistakes like themselves. All mistakes are not due to stupidity, and mistakes often are as important as correctnesses, because they give rise to new considerations. For this last reason, the early manuscripts of Leibniz, for example, are well worth study by beginners in the calculus.

In this series of scientific classics the requirements of students and teachers are kept in mind. As to the form of these little volumes: they are printed on good paper, and with margins so wide that notes can be conveniently written upon them by the students. They are produced in such a size that they will go easily into an outside pocket, and are bound in cloth which is durable, and pleasant to the hand and eye.

The translations are carefully made, thoroughly revised, and the notation modernised. Of course, a note to this effect is added when necessary, because the historical interest must be kept in view. Any figures are included in the text

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Lastly, notes which explain difficult passages or give references to earlier and later important work on the same subject are added by competent authorities, and it is hoped that by this the value of the scientific classics will be greatly increased. Where it seems advisable, the places where the pages of the original publication begin and end are indicated, so that references to the originals may easily be verified in these editions.

The prices have been kept as low as possible, but the first consideration has been serviceability and goodness of material.

The series will include memoirs from pure mathematics and all the natural sciences and philosophy; and a special feature will be the inclusion of quite modern researches. Among the authors whose work will be published are: Maxwell, Helmholtz, Gauss, Volta, Ohm, Oersted, Kirchhoff, Carnot, Cauchy, Dirchlet, Riemann, Weierstrass, Cantor, Newton, Leibniz, Descartes, Bolzano, Lagrange, Laplace, Lavoisier, Mayer, Joule, Neumann, Zeno, Voltaire, Diderot, and many others.

The following volumes have been already published:—

No. 1. Contributions to the Founding of the Theory of Transfinite Numbers. By GEORG CANTOR. Translated, and provided with an Introduction and Notes, by PHILIP E. B. JOURDAIN, M.A. Cloth. 3s. 6d. net.

This volume contains a translation of the two very important memoirs of Georg Cantor on transfinite numbers which appeared in 1895 and 1897. These memoirs are the final and logically purified statement of many of the most important results of the long series of memoirs begun by Cantor in 1870. A very full historical account of this work and the work of others which led up to it is given in the introduction, and the notes at the end contain indications of the progress made in the theory of transfinite numbers since 1897. This book is a companion volume to Dedekind's *Essays*, of which a translation has been issued by the present publishers.

No. 2. Selections from the Scottish Philosophy of Common Sense. Edited by G. A. JOHNSTON, M.A., Lecturer in Philosophy at Glasgow University. Cloth. 3s. 6d. net.

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This volume consists of extracts from the works of the chief representatives of the Scottish Philosophy of Common Sense. The selections have been made, for the most part, from Reid's *Inquiry into the Human Mind on the Principles of Common Sense*, and *Essays*; but these have been supplemented by important or typical passages from Stewart, Beattie, and Ferguson. Most of the selections are metaphysical or psychological, but ethical doctrines have not been neglected. In the introduction an attempt has been made to estimate Reid's historical importance in connection with the Locke-Berkeley-Hume development of thought, and in relation to Kant's "answer to Hume." The Scottish Philosophy contains much that is interesting and valuable, but it has not been readily accessible to the student, partly because the writings of its representatives are prolix to a degree, and partly because their books are not easily obtainable. These facts constitute the sufficient justification of such a volume as this. And at a time when much interest is being taken in New Realism, it may not be out of place that some attention should be paid to the Natural Realism of the Scottish Philosophy.

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